THREE ESSAYS ON FAMILY POLICY

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[This dissertation examines issues in family demography and the role of public policy in shaping life course transitions including educational attainment, marriage, and family formation. The first two chapters focus on the Earned Income Tax Credit, one of the largest cash transfer programs in the United States. Chapter one analyzes the impact of the EITC on the educational attainment of youth from low socioeconomic status families. Variation in state EITC policies is used to analyze the impact of increased household income on children's educational attainment. Results suggest that a $1,000 increase in state EITC generosity increases college enrollment among 18-23 year olds by 1 percentage point and increases college enrollment by 0.3 of a percentage point, a 10% increase in college completion among this group. Chapter two analyzes the marriage penalties associated with the EITC. First, I simulate a marriage market to predict the earnings of potential spouses and how a spouse's earnings impact EITC benefits. Multinomial regression models are then used to assess how an expected loss in EITC benefits upon marriage affects one's likelihood of marrying or cohabiting. Results suggest that approximately 65% of EITC recipients can expect to lose some of their EITC benefits upon marriage. The average EITC recipient can expect to lose approximately half of her EITC benefit, or about $1,050. A $1,000 expected loss in EITC benefits upon marriage is associated with a 1.8 percentage point decline in the likelihood of marrying, and a 1.1 percentage point increase in the likelihood of cohabiting. Chapter three (co-authored with Kelly Musick) examines variation in family formation among college graduates. We explore differences in the rates of childlessness among college-educated women across academic disciplines and examine potential mechanisms that may account for such differences. We find that women who major in health-related fields have the lowest levels of childlessness at around 16% by age 44, while women who major in the arts and humanities have the highest rates of childlessness, at 25%. We find that these differences are correlated with differences in early marriage patterns among women in these fields, as well as traditional gender role attitudes.]

BIOGRAPHICAL SKETCH

Katherine received her Bachelors of Arts degree in Economics and Psychology from Wesleyan University. Prior to starting her PhD at Cornell, Katherine worked as a research assistant at the Urban Institute in the Income and Benefits Policy center, where she worked on issues relating to Social Security reform and population modeling projections. While living in DC, Katherine also volunteered to prepare tax returns for low-income families with the DC EITC Campaign. It was there that she learned about the Earned Income Tax Credit and the extent to which people rely on the EITC to support their families. Seeing how such a large program affected the lives of so many people inspired her to focus her dissertation on the impact of the EITC on marriage and education outcomes.

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Kelly and I met in my first year of graduate school when I took her family demography seminar. It was in that class that I started writing what would become my third chapter, which Kelly graciously offered to help re-write and submit for publication. I am very grateful to Kelly for offering to help rewrite that paper three years ago, and for all of her helpful advice over the years.

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CHAPTER 1

THE EFFECT OF INCOME ON EDUCATIONAL ATTAINMENT: EVIDENCE FROM STATE EARNED INCOME TAX CREDIT EXPANSIONS

The gap in college enrollment rates between children growing up in low-income and high-income households has been well documented in the literature (Bailey and Dynarski 2012; Long 2008). As of the early 2000s, children growing up in the bottom income quartile were 50 percentage points less likely to enroll in college compared to children growing up in the highest income quartile (Bailey and Dynarski 2012). While some suggest that this gap results from a lack of academic preparedness for higher education among the poor (Carneiro and Heckman 2002), there also has been evidence that financial constraints play an increasingly important role in college enrollment decisions in recent decades (Belley and Lochner 2007). The extent to which the cost of college affects college-going among children is much-debated in the literature. There is a long literature examining the effects of household income and financial aid programs on educational attainment with some studies finding little or no impact of household income on college enrollment (Cameron and Taber 2004; Carneiro and Heckman 2002; Hilger 2013), while others have found significant increases in college enrollment associated with increases in household income (Abraham and Clark 2006; Belley and Lochner 2007; Dynarski 2003; Kane 2007; Lovenheim 2011). [1]These studies focus on the short-run impact of increasing resources near the time of college-going, but there is also a long literature pointing to the importance of early childhood interventions on later life outcomes (Carneiro and Heckman 2003; Deming 2009; Chetty et al. 2011).

[1] Kane 1994 finds that while children from low-income households are responsive to the costs of college, increasing financial aid policies may not increase college enrollment rates if individuals do not have full information about the amount of financial aid available to them.

One potential source of exogenous variation in household income that has received little attention in past work is the Earned Income Tax Credit (EITC). The EITC is the largest cash transfer program in the U.S., redistributing over $60 billion dollars to low-income households in 2010 (Tax Policy Center 2013). The EITC is designed to subsidize low-wage work, with recent estimates suggesting that the EITC helped lift 3.1 million children out of poverty in 2011 (Center on Budget and Policy Priorities 2011). The EITC is a particularly interesting income intervention program because it potentially affects college-going through multiple pathways: by increasing household resources both during early childhood and around the time of college-going. While most families that receive the EITC have young children, the EITC also incentivizes college-going by providing conditional cash transfers to older children--those who are full-time students between the ages of 19 and 23. While there have been numerous studies exploring how the EITC affects low-income households (see Hotz and Scholz (2003) or Meyer (2010) for a review), there has only recently been a focus on the effects of the EITC on the children of EITC recipients (e.g. Dahl and Lochner (2012); Hoynes, Miller, and Simon (2012)).

This paper examines the effect of the EITC on the educational attainment of children of EITC recipients, making three primary contributions to the literature. First, this paper adds to the literature on the impacts of the EITC on children of EITC recipients. Given the size of this program, its impact on educational attainment is policy interest in its own right. By using variation in state EITC generosity, this is the first analysis to estimate both the short-run impact on college investment of the EITC as a conditional cash transfer to college-aged children and the long-run impact of the EITC as an early childhood income intervention. Second, this work contributes to the literature on the effects of household resources on college enrollment by exploiting a new source of exogenous variation: expansions of state EITC benefits. Third, this paper provides direct evidence on when in a child's life income shocks matter the most for educational attainment by examining the effects of EITC expansions for younger versus older children.

In this paper, I use a difference-in-differences analysis with variation in treatment dosage to evaluate the impact of changes in state EITC benefits on the educational attainment of disadvantaged youth. Throughout the 1990s and 2000s, several states implemented their own EITCs that piggy-back off of the federal EITC. As of the 2012 tax year, 23 states and the District of Columbia offered some form of a state EITC. For the vast majority of these state credits, federal eligibility determines state eligibility and credits are typically a fixed percent of the federal benefit, ranging from 3--45 percent. Many states also changed the generosity of their benefits over time, providing variation in both the timing of implementation and the timing of state EITC benefit level changes. I estimate the impact of increases in household income on college enrollment by comparing outcomes of individuals before and after changes in state EITC benefits to those of children living in states that do not experience changes in EITC benefits. These control states are predominantly those that never implement a state-level EITC, but they may not be adequate counterfactuals if EITC-implementing states experience secular increases in educational attainment for all children.[2] To address this concern, I also use a triple-difference strategy comparing changes in outcomes of children of likely EITC-recipients to educational trends of children from more affluent households within the same state, relative to changes in outcomes of children living in states that never implement EITCs. Outcomes of interest include high school completion rates, years of completed schooling, college enrollment, and college completion. Variation in the timing of state EITC expansions also will allow for evaluation of how effects vary based on the length of time an individual is exposed to a state EITC. If the EITC increases educational attainment by providing a conditional cash transfer to college-aged

[2] In years when states do not change EITC generosity, those states contribute to the identification of year effects, even if they have implemented state-level EITCs.

individuals, we would expect the effect size to be independent of the length of time one is exposed to a state EITC, so long as there is a state benefit while the child is in college. If instead, the EITC increases educational attainment by providing extra income to households with young children, thus alleviating long-run credit constraints, we would expect effects to increase with length of exposure to state EITCs.

To conduct this analysis, I use data from the Survey of Income and Program Participation (SIPP), pooling panels from 1990 through 2008. I focus on 18--23 year olds for this analysis, using parental educational attainment as a proxy for EITC-eligibility. Individuals living with parents that have no schooling beyond a high school degree will be considered EITC-eligible, while all others will be considered non-eligible.[3] Results are quite similar when actual eligibility is used instead of parents' education, discussed in the robustness checks section IV.C, below} This results in a lower-bound estimate, since some of the `non-eligible' individuals may have indeed received the EITC as children. Results using only the EITC-eligible sample can be thought of as an upper bound estimate since they do not control for state-specific secular trends in educational attainment other than time-invariant state-level effects.

My results indicate that following a $1,000 increase in EITC benefits, 18--23 year old children from low-educated households are more likely to complete high school (2 percentage points on a base of 70 percent), have more years of schooling (0.11 of a year on a base of 11.97 years), are more likely to have ever enrolled in college (2.5 percentage points on a base of 41 percent), and are more likely to complete a bachelor's degree (0.7 of a percentage point on a base of 3 percent). Controlling for state-specific trends in educational outcomes using children from highly-educated households yields smaller, but more precisely-estimated positive results. Compared to children growing up in highly-educated households, a $1,000 increase in the

[3] Education is often used in the EITC literature to proxy for eligibility, due to concerns of potential endogeneity of benefit-claiming and income levels to the outcomes of interest (Baker 2008; Baughman and Dickert-Conlin 2009; Meyer 2010; Celik 2011).

maximum federal and state EITC benefit increases college enrollment among children from low-educated households by 0.7 percentage points (on a base of 26 percent), and increases the likelihood of having a bachelor's degree by 0.3 percentage points (on a base of 3 percent). I find that these effects are much larger for children who were younger than 12 years old at the time their state implemented an EITC. I also find no effect of EITC expansions on children who were college-aged at the time of implementation, for whom the EITC acts as a form of financial aid. This finding suggests that the EITC increases college enrollment primarily by providing income transfers to households with young children, allowing children to benefit from several years of increased household income. These results support previous work emphasizing the importance of early childhood investments for later life outcomes (e.g. Carneiro and Heckman (2003); Duncan, Ludwig, and Magnuson (2007) Chetty et al. (2011)).

The paper is structured as follows. In Section I, I discuss the structure of the EITC and how it might affect education outcomes. Section II describes the data, Section III discusses the empirical strategy, and Section IV contains results. Section V concludes.

I. EITC Background

Since its inception in the 1970s, the EITC has undergone several expansions at both the federal and state level. As of the 2012 tax year, the EITC provided a benefit worth up to 45 percent of earnings for households with three or more children. It is also fully refundable, so households with no tax liability receive the EITC benefit as a refund. In addition to the federal benefit, 23 states and the District of Columbia have their own EITCs, which increase the total benefit by 3-45 percent of the federal benefit. States implemented their own EITCs beginning in the late 1980s, but the majority implemented credits in the late 1990s and early 2000s. Table 1 shows a list of states that have ever implemented EITCs, the year of implementation, the benefit level at the time of implementation, and the benefit level as of the 2011 tax year.

While there is quite a bit of variation in the timing of implementation of state EITCs, several states also change their benefit level over time. For instance, New York implemented an EITC in 1994 worth just 7.5 percent of the federal EITC. As of the 2011 tax year, New York had increased the value of its EITC to 30 percent of the federal EITC. Other states have reduced or eliminated their EITCs. Colorado, for instance, had an 8.5 percent EITC in 1999 but suspended it in 2003 due to lack of funding.

Pooling all states that implemented EITCs, Figure 1 shows the average maximum combined federal and state EITC benefit in thousands of (2011) dollars over time since state EITC implementation. Before states implemented EITCs, the maximum federal EITC and the combined federal and state EITC were one and the same and grew only due to real changes in the federal maximum benefit. When the average state in the sample implemented its EITC, the maximum federal EITC benefit was approximately $4,000. Fifteen years after implementation, the average maximum federal and state EITC had grown to nearly $7,000 in real terms, while the maximum federal EITC alone was worth approximately $5,000 in year 2011 dollars.

The EITC is calculated based on the earnings of the head of household and spouse (if married) and the number of children living in the household. The EITC benefit structure has three segments: a phase-in region, where benefits increase as earnings increase; a plateau region, where benefits do not change with increases in earnings; and a phase-out region, where benefits decrease for every extra dollar earned. The steepness of the phase-in and phase-out segments depends on the number of children living in the household. For a household with two children, the slope of the phase-in region is 0.40, so every dollar of earnings increases the EITC benefit by 40 cents. Once earnings reach a certain threshold, the EITC benefit is constant until earnings reach a second threshold, at which point benefits are phased out at a rate of 21 cents per dollar for a household with two children. Figure 2 illustrates the structure of the federal EITC for the 2011 tax year.

While there is a small benefit for households with no children, households with children represent 77 percent of EITC claims and over 97 percent of dollars spent on the EITC (Tax Policy Center 2012). A family can claim up to two (three for tax years 2009 and beyond) qualifying children on their tax forms. Qualifying children are defined as related children under the age of 19 that live in the household for at least 6 months of the year. Children over the age of 18 are also eligible up until age 24 if they are full-time students for at least 5 months of the year.[4] Because the benefit is contingent upon full-time enrollment, the EITC can be thought of as a form of financial aid. While maximum EITC benefits are over $5,000 for households with two or more children, childless individuals are eligible for less than $500 as of the 2011 tax year. A couple earning $20,000 in 2011 would be eligible for the maximum benefit of $3,094 claiming one child but would be ineligible for any EITC benefit without a dependent. If that family lived in a state that supplemented the federal EITC, they could earn up to an additional $1,392, bringing the total gain in EITC benefits to $4,486 (2011$) for each year their child remained in school after age 18. With the addition of other types of financial aid that this family would likely qualify for, these benefits could help finance their child's higher education.[5]

I.A The EITC and Educational Attainment

There is some literature that suggests that low-income children do not attend college at the same rate as high-income children not because of short-term financial constraints, but because they

[4] Children living away at school are considered living in the household during those months and may be claimed as dependents. `Full-time' status is defined by the institution. Students can be enrolled in any type of academic institution; secondary, or post-secondary (Internal Revenue Service 2012).}

[5] Net costs of a public four-year institution for the 2007-08 school year was $10,000 for individuals in households that earned less than $32,500 in 2006. Net costs of a public two-year institution for the 2007-08 school year was $6,000 for individuals in households that earned less than $32,500. Net costs reflects tuition, room and board, and other costs, subtracting grant and loan aid. Net costs do not take into account tax credits or deductions (College Board 2012).

lack sufficient academic preparation to attend college (Carneiro and Heckman 2002; Cameron and Taber 2004). Carneiro and Heckman (2002) argue that obtaining loans to pay for college in the short-run is relatively easy, but rather the so-called 'long-term' credit constraints--the inability of parents to borrow against their children's future earnings to pay for better schooling throughout their children's lives--are what limits college enrollment among low-income households. They argue that gaps in academic preparedness, not financial aid, are what accounts for the majority of the gap in college enrollment rates between individuals from high-income backgrounds and those from low-income backgrounds. More recent evidence, however, has shown that gaps in college enrollment rates remain even after controlling for ability differences between low-income and high-income households (Belley and Lochner 2007), suggesting that short-run financial constraints are playing an increasingly important role in college enrollment decisions. The EITC could help alleviate both of these types of constraints by providing short-run financial aid for children during the college-going years and by providing extra income to low-income households with young children, improving the quality of schooling children receive throughout their lives.

There are several features of the EITC that make it a potentially important program for increasing the educational attainment of low-income youth. The EITC is a substantial credit, worth nearly $6,000 in 2012 for households that earned between $13,000 and $22,300. Unlike traditional welfare programs, there is no lifetime limit to the number of years households can claim the EITC. Because families tend to claim the EITC for consecutive years, state and federal policies that increase the maximum EITC may produce several years of higher household income for low socioeconomic status households.[6] Children of EITC recipients may benefit from several years of increased income throughout their childhoods, and these benefits may improve their

[6] The average family that is ever eligible for the EITC will typically claim it for 3 years (Ackerman, Holtzblatt and Masken 2009).

quality of schooling or the quality and quantity of parental investments in them, thus increasing the likelihood of completing high school and enrolling in college. Several studies have analyzed the types of purchases made with EITC benefits, suggesting that many recipients use the money to pay down debt (Smeeding et al. 2002; Tach and Halpern-Meekin 2013), save money for their children (Tach and Halpern-Meekin 2013), and make other purchases to generally improve their social standing (Smeeding et al. 2002). This evidence is consistent with EITC payments being used to invest more in children.

There has been some evidence suggesting that the EITC increases contemporaneous test scores of children aged 5 to 15 (Dahl and Lochner 2012). Using a federal expansion of the EITC for two-child households, this study shows that a $1,000 increase in benefits increased math and reading test scores on the Armed Services Vocational Aptitude Battery (ASVAB) by 6 percent of a standard deviation. While these effects suggest that the EITC does have a positive impact on the outcomes of children of EITC recipients, it is not clear whether these effects persist, and whether these test score gains lead to gains in long-run educational attainment. Many policy interventions that improve the short-run test scores of children have been shown to fade out quickly after the intervention (Currie and Thomas 2000; Kane and Staiger 2008; Jacob, Lefgrin, and Sims 2010), although effects on longer-term outcomes such as high school graduation and college enrollment have been found years after an intervention has ended (Krueger and Whitmore 2001; Ludwig and Miller 2007; Deming 2009; Chetty et al. 2011), increasing the need for further research to assess whether the test score gains from the EITC translate into improvements in longer-term outcomes.

In addition to providing extra income to households with young children, claiming rules for dependent children create direct financial incentives for families to send their children to college. The EITC requires that in order to claim a child as a dependent, she must be under the age of 18 at the end of the year or be under the age of 24 and a full-time student. Since there is a very small benefit available to households with no children, this conditional cash transfer provides a strong incentive for EITC-eligible households to encourage their children to go to college. The EITC has an additional advantage over traditional forms of financial aid in that it does not require the Free Application for Federal Student Aid (FAFSA) to determine eligibility, and income from the EITC is not included in calculations for financial aid eligibility.[7] Recent research on the FAFSA suggests that its complexity serves as a significant barrier to college enrollment and that simplifying the application would increase the probability that a low-income child attends college (Dynarski and Scott-Clayton 2006; Dynarski and Scott-Clayton 2008; Bettinger et al. 2012). The EITC, on the other hand, requires only an additional sheet to attach to the federal 1040 tax form.[8] Perhaps because the EITC is easy to claim, and many low-income taxpayers utilize free services or professional tax preparers to complete their taxes, the EITC has a fairly high participation rate compared to other welfare programs, at over 80 percent of eligible taxpayers (Chetty, Friedman, and Saez 2012).

The conditional cash transfer aspect of the EITC is quite similar to the Social Security Student Benefits Program (from here on referred to as SSBP) that provided financial aid to college students of retired or deceased parents. Previous research suggests that the SSBP increased college enrollment among children with deceased fathers by 3.5 percentage points for every $1,000 of aid (Dynarski 2003). While the program provided generous benefits to qualifying individuals, Dynarski (2003) shows that at its peak, only 12 percent of full-time

[7] Most EITC-eligible households would have 0 expected family contributions from the FAFSA and would likely be eligible for many federal grants and loans. Though take up of financial aid is not 100 percent, it is not clear that small increases in EITC generosity would necessarily generate large, short-run increases in college enrollment since many EITC-eligible families would have access to many other forms of financial aid.

[8] The federal tax code already provides financial aid to college students through the Hope Tax Credit and the Lifetime Learning Credit, which are worth around $2,000 in non-refundable credits. Long (2004) shows that because these credits are non-refundable, they are more likely to help middle-income households and households where individuals would have attended college regardless of the credit.

college students received the benefit, and only 5 percent of all children experienced the death of their father before age 18. The EITC presents a similar type of cash transfer as the SSBP, providing conditional cash transfers through the tax code for students up until age 24. Compared to the SSBP, the EITC is likely to reach a much larger group of children, with some recent evidence suggesting that half of households with children will claim the EITC at some point over an 18-year period (Horowitz and Dowd 2011). This analysis also provides a more recent picture of how college enrollment of low-income children is affected by changes in household income.[9]

Several other studies have found positive effects of household resources on college enrollment, using a variety of definitions of income to assess the impact on college enrollment. Recent studies have used variations in state-specific scholarship programs (Dynarksi 2000; Abraham and Clark 2006; Kane 2007}, housing wealth (Lovenheim 2011), and federal regulations concerning drug offenders and eligibility for federal financial aid (Lovenheim and Owens 2013) to examine the impact of income on college enrollment, all finding significant, positive effects of income on college enrollment.

In a recent study by Manoli and Turner (2014), the authors exploit variation in income generated by kinks in the EITC benefit structure to analyze the impact of an increase in income during one's senior year of high school on college enrollment the following fall. The authors find a 2-3 percentage point increase in college enrollment associated with a $1,000 increase in household income. This strategy of comparing EITC-recipients just above and below the kink point estimates the effect of an increase in income on college enrollment, but does not capture the total effect of the EITC on college enrollment. In addition to providing extra household income just prior to college-going, the EITC may also increase college enrollment by providing resources to households with young children. Further, the rules regarding qualifying children

[9] The SSBP has not been in existence for more than 30 years, and more recent evidence suggests that financial constraints are playing an increasingly important role in college-going decisions for low-income households (Belley and Lochner 2007).

increase the incentives for low-income families to keep their 19--23 year old children in school, the effect of which cannot be directly evaluated in Manoli and Turner (2014). My analysis is the first to estimate both the short-run impact of the EITC (increases in income around college-going age) and the long-run impact (being exposed to a larger EITC throughout childhood) on subsequent college-going.

II. Data

The data I use in this analysis come from the Survey of Income and Program Participation (SIPP) panels from 1990 through 2008 and the Current Population Survey March Supplement (CPS) from 1992-2011.

II.A Survey of Income and Program Participation

The SIPP is a nationally-representative household survey that follows households for up to 48 months and interviews each household member about their employment, educational attainment, and income sources. The data I use for this analysis come from pooling eight different surveys from 1990--2008, which cover the years between 1990--2011. Each survey follows households for at least 32 months, with some surveys following households up to 48 months. The short panel nature of the SIPP allows one to observe individuals living in the household in the first month of the interview and follow them throughout the survey, even if they later leave the household. Individuals living away from the household for schooling are considered living in the household with their parents. Using the SIPP data, I examine the educational attainment of 18-23 year olds who were living with their parents in the first month of the interview, evaluating their educational attainment in each March of the survey, to coincide with the observation month of the CPS March Supplement. Outcomes of interest include current and past college enrollment, years of schooling, and completion of various degrees: high school, associate's and bachelor's degrees. Restricting the sample to 18--23 year olds living with a parent in the first month of the survey produces a sample of 81,724 person-year observations (25,337 unique individuals).

I next proxy for EITC-eligibility using parents' education. Specifically, I consider the sample of 18--23 year olds who live in households where neither parent in the household has schooling beyond a high school degree, a sample of 31,130 observations. Because I can only observe 3 to 4 years of family income using the SIPP, I can only assess actual EITC-eligibility when individuals are 18--23 years old, but the EITC tends to go to families with younger children (Ackerman, Holtzblatt, Masken 2009). By using actual EITC-eligibility for 18--23 year olds, I may be capturing a particularly disadvantaged group of individuals. Additionally, many 18--23 year olds that are not living in EITC-eligible households may very well have received the EITC as young children. Because of this, as well as concerns of endogeneity of eligibility to education outcomes, I prefer to use parents' education as a proxy for eligibility, which is likely a better indicator for childhood EITC-eligibility.[10] I refer to this group as the 'high-impact' sample. As a robustness check, some analyses were also conducted using actual EITC eligibility in the first year of the SIPP survey. Results are discussed in Section IV.C below, and are qualitatively quite similar to results generated when using parental education as a proxy for EITC-eligibility.

While this strategy of matching children to their parents in the first month of the survey allows me to capture approximately 70 percent of the sample of 18--23 year olds living with a parent, the individuals not observed living with a parent are unlikely to be a random sample. Children who do not enroll in college right after high school are less likely to live with their parents, so focusing on individuals who live at home may over-represent individuals enrolled in school. As this is more of a concern among the older individuals in the sample, I also conduct

[10] Approximately 55 percent of individuals living with parents that had no schooling beyond a high school degree were eligible for the EITC in at least one year of the SIPP survey, as determined by household income and the number of children in the household.

additional analyses restricting the sample to 18--20 year olds. Restricting the sample to this narrower age range allows me to observe approximately 85 percent of 18--20 year olds in the SIPP.[11]

While the SIPP survey allows for observation of individuals over time, even if they leave the household, the survey also has limitations. Prior to the 2004 survey, the SIPP did not have individual state identifiers for a few states. Though most of the states without individual identifiers were quite small in population (e.g. Vermont and Maine), this may pose an issue in determining who is treated by a state EITC.[12]

II. B. Current Population Survey

To ensure that the results are not unique to the sampling design of the SIPP, I also use the CPS March Supplement. The CPS provides a much larger sample of 18--20 year olds than the SIPP---roughly six times the number of unique individuals aged 18--20 in the SIPP. The CPS March Supplement is a cross-sectional, nationally-representative household survey that collects information on annual earnings from the prior calendar year. It also collects information on demographic characteristics such as race, educational attainment and enrollment at the time of survey. Everyone currently living in the household is surveyed, as are individuals who usually live in the household but are currently living away for school. In contrast to the SIPP, which follows households over time, the CPS March Supplement observes households only once, so

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[11] Differences between individuals living with a parent in the beginning of the survey and those not living with a parent can be found in Appendix Table 1. In results not shown, but available upon request, there was little evidence that the likelihood of being in the sample of individuals living with at least one parent changed as a function of state EITC generosity.

[12] Prior to the 2004 survey, for instance, one could not distinguish between residents living in North Dakota, South Dakota, Wyoming, or in some years, Iowa; while individuals living in Maine and Vermont were also indisguishable. North Dakota, South Dakota, and Wyoming never implement state EITCs during this time period, so all individuals residing in those states serve as part of the control group. Maine and Vermont both have EITCs, and they were established at different points in time and are of different generosity levels. Because of this, I exclude individuals who lived in either Maine or Vermont for the survey years prior to 2004. The number of individuals living in Maine and Vermont in those years was quite small, and is unlikely to substantially impact the results.

individuals must be living with at least one parent at the time of interview in order to observe parental education. Like the SIPP, this creates concerns regarding those who are not living at home, which is a bigger issue in the CPS since individuals are only observed at one point in time. Because of this concern about selection into who lives with a parent, I restrict my analysis to individuals 18--20 years old at the time of the survey---a sample of 97,123 individuals, 36,063 of whom are in the high impact sample. By restricting the analysis to individuals aged 18--20 who live with at least one parent, I observe approximately 70 percent of the sample of all 18--20 year olds in the CPS.

II. C. Summary Statistics

Table 2 presents summary statistics for the sample of 18--23 year olds in the SIPP living with at least one parent at the start of the survey in the high-impact sample. I present summary statistics for individuals living in states that never implement EITCs and states that do implement EITCs, separately analyzing the years before and after implementation for the states that implement EITCs. The last column shows the change in characteristics before and after state EITC implementation compared to individuals living in states that never implement EITCs.

Before implementing EITCs, individuals living in states that eventually implement EITCs have better schooling than individuals living in states that never implement EITCs. They are about 3 percentage points more likely to be currently enrolled in college (26 percent compared to 23 percent), have about 0.10 more years of schooling, and are about 4 percentage points more likely to have ever enrolled in college. They are also 4 percentage points more likely to have completed a high school degree. Comparing the changes in characteristics before and after changes in state EITC implementation to states that never implement EITCs (last column), individuals living in states with EITCs are about 5 percentage points more likely to be currently enrolled in college after the implementation compared to individuals living in states that never implement EITCs. They also have 0.19 more years of schooling, are 5 percentage points more likely to have ever enrolled in college, and are about 1 percentage point more likely to have a bachelor's degree.

Table 3 shows descriptive statistics in the same format as Table 2 for the other samples of interest: the 18--20 year olds from the SIPP and the 18--20 year olds from the CPS. Changes in the outcome variables of interest are very similar among the 18--20 year olds in the SIPP compared to the 18--23 year olds. For the 18--20 year old SIPP sample, there is a 5 percentage point increase in college enrollment after states implement EITCs, compared to individuals who live in states that never implement EITCs. There is no change in the number of years of schooling or the likelihood of having a high school degree. Somewhat larger results are shown using the CPS. After implementing an EITC, individuals in the CPS are 7.5 percentage points more likely to be currently enrolled in college, have about 0.19 years of schooling, are 5 percentage points more likely to have completed high school, and are 7.5 percentage points more likely to have ever enrolled in college, compared to individuals living in states that never implement EITCs. The larger effects in the CPS could be due to larger sample sizes than the SIPP and thus more precise estimates, or they could be due to potential sample selection where 18--20 year olds in the CPS are more likely to be living at home if they are enrolled in college.

While Table 2 and 3 indicate that state EITC implementation is positively associated with educational attainment, other demographic characteristics change as well. The share of blacks in the 18--23 year old sample declines dramatically following state EITC implementations, and the share of women in the sample increases. The increases in educational attainment observed may be spurious if sample characteristics change at the same time as changes to the EITC. To test whether the demographic characteristics change as a function of changes in the state EITC benefit, I separately regress each demographic control on the maximum federal and state EITC in a given state and year controlling for state and year fixed effects. Results can be found in Appendix Table 2. Each cell represents a separate regression, and the coefficients displayed illustrate the effect of a $1,000 increase in the maximum federal and state EITC on each demographic control. The first three columns use only the high-impact sample, while the second three columns use children from the low-impact sample to test whether the changes in demographic controls occur differentially for the high-impact sample compared to the rest of the population. Once including state and year fixed effects, very few of the demographic controls appear to change as a function of the EITC in either the high-impact sample or in the triple-difference analysis, alleviating some concern that changes in education outcomes are driven by changes in the composition of the sample. However, in the triple-difference analysis children in the high-impact sample are significantly less likely to live in two-parent households when the EITC increases by $1,000, indicating that the EITC may discourage marriage among the low-educated population. [13] For all other demographic controls, the triple-difference analysis confirms that, once adjusting for state and year fixed effects, there is very little change in observable characteristics associated with changes in the EITC.

III. Empirical Method

To analyze how the EITC affects the educational attainment of children from low-educated families, I employ a series of difference-in-differences estimators that examine the changes in high school graduation rates, years of schooling, college enrollment, and degree completion following changes in state EITC benefits. I create a treatment variable equal to the maximum combined federal and state EITC that a household with two children could receive in a given

[13] The relationship between the EITC and marriage has been examined, although there is little evidence that the EITC significantly alters marriage behavior, see Dickert-Conlin and Houser 2002.

state and year.[14] All individuals in the analysis are assigned this value, which can be thought of as the maximum potential EITC benefit for an individual living in a given state in a particular year. For instance, in the 2011 tax year, the maximum federal EITC for a household with two children was $5,112. Individuals living in Washington, DC for the 2011 tax year also were eligible for an additional $2,045 (DC had an EITC worth 40 percent of the federal EITC in 2011), so the maximum combined federal and state EITC for a household with two or more children was $7,157 in Washington, DC in 2011. All values were then adjusted for inflation and reported in year 2011 dollars, using the consumer price index.

I begin by examining the effects of the maximum federal and state EITC on individuals in the high-impact sample only, comparing outcomes of individuals living in states after an increase in the EITC by $1,000 to outcomes of individuals who lived in states before the increase occurred, relative to changes in outcomes among individuals in the untreated states and years. Using linear regression models, I estimate equations of the following form:

$$Y_{i,s,t} = \beta_0 + \beta_1 EITC_{s,t} + \gamma X_{i,s,t} + \varphi V_{s,t} + \theta Z_s + \alpha W_t + \varepsilon_i \quad (1)$$

where i indexes individuals, s indexes states, and t indexes years. $\boldsymbol{Y_{i,s,t}}$ is the outcome variable of interest: college enrollment, high school completion, years of schooling, and degree completion. As discussed above, $EITC_{s,t}$ is the maximum federal and state combined EITC benefit in a given state and year for a household with two children, in thousands of dollars. The coefficient on this term identifies within-state changes in the outcome variables of interest associated with a $1,000 change in the state EITC benefit. $\boldsymbol{X_{i,s,t}}$ is a vector of

[14] I use the maximum benefits for a two-child household rather than the maximum benefit for the actual number of children in a particular household to avoid issues of endogeneity of family structure to EITC policy changes.

personal characteristics that includes age, race, gender, number of male and female siblings living in the household, how many times the respondent appears in the sample (up to 4), and whether both parents are living in the household. $\boldsymbol{V_{s,t}}$ includes state-by-year economic indicators, including the minimum wage and the unemployment rate. $\boldsymbol{Z_s}$ and $\boldsymbol{W_t}$ are state and year fixed effects, respectively. State fixed effects control for time-invariant state-level characteristics, such as political ideology. Year fixed effects control for changes that affect all states, such as the federal unemployment rate or expansions to the federal EITC.[15] Both of these fixed effects are identified because not all states implement or change EITCs at the same time.

States that never implement EITCs may not be an adequate comparison group for states with their own EITC benefits. While there is considerable variation in the timing of state EITC changes, there may be other changes in state-level policies that occur at the same time as changes to state EITCs that affect all college-aged individuals, which would confound the effects of the EITC on educational attainment. For instance, Table 2 indicated that the share of women in the EITC-implementing states increased following state EITC implementation. If there are secular increases in female college enrollment in states that have EITCs, comparing outcomes of individuals in states that have EITCs to outcomes of individuals in states that do not have EITCs may generate a spurious association between the EITC and education outcomes. State fixed effects do not adequately address this concern because they only control for time-invariant characteristics particular to each state. In another set of analyses, I use a series of triple difference models to provide a different counterfactual for EITC-eligible children in states that implement EITCs. In this set of analyses, I include all individuals, regardless of their parents' educational attainment, and compare the outcomes of children in the high-impact sample to those of children not likely to be affected by changes in the EITC---those living in highly-educated

[15] The federal EITC was expanded for two-child households gradually from 1991 to 1996.

households. This model will take a very similar form to (1), but will include individuals from the low-impact sample to control for other state-level trends in education outcomes. This model will include an indicator for whether the respondent is in the high-impact sample, as well as an interaction of the maximum EITC with whether the respondent is in the high-impact sample:

$$Y_{i,s,t} = \beta_0 + \beta_1 EITC_{s,t} + \beta_2 ELG_{i,s,t} + \beta_3 EITC_{s,t} * ELG_{i,s,t} + \gamma X_{i,s,t} + \varphi V_{s,t} + \theta Z_s + \alpha W_t + \varepsilon_i$$

(2)

where $ELG_{i,s,t}$ is equal to one if the individual lives in a household where neither parent has schooling beyond a high school degree, and zero otherwise. β_3 represents the key independent variable of interest: the difference in the outcome variable between children in the high-impact sample (those with low-educated parents) and the low-impact sample (those with highly-educated parents) associated with a $1,000 increase in potential EITC benefits. It is worth noting that some individuals in the low-impact sample did live in households where their annual income placed them in the EITC-eligibility range, so some of these individuals may have actually received the EITC.[16] Estimates from these regressions may be thought of as a lower-bound, as some individuals in the comparison group may also benefit from the credit.

To address concerns that secular trends in educational attainment among individuals living in states that have EITCs may cause spurious associations between EITC changes and educational attainment, Figure 3 plots an event study of the effect of the maximum federal and state EITC on changes in the outcomes of interest for the years leading up to, and following the state EITC implementation. The coefficient plotted is the triple-interaction of the maximum federal and state EITC benefit with an indicator for being in the high-impact sample, for each

[16] Approximately twenty percent of the sample of individuals in the low-impact sample were eligible for the EITC as determined by their household income and the number of children residing in the household.

year before and after implementation. Following state EITC implementation, interacting the maximum federal and state EITC with time-since-implementation indicators illustrates the effects of within-state changes in the generosity of EITC benefits on changes in the outcome variables for each year since implementation. Each graph represents a separate regression---one for each outcome of interest. All regressions include the triple-interaction of EITC eligibility, EITC generosity, and years since implementation, along with demographic controls, state fixed effects, and year fixed effects. Standard errors are clustered at the state level. The lighter gray bars represent the 95% confidence intervals. Each graph is limited to the 10 years before and after state EITC implementation, as there are few states that have more than 10 years of either a pre- or post-implementation window. The year before implementation is omitted as the reference category. The graphs show little evidence of pre-trends in any of the outcomes of interest as a function of EITC generosity, although confidence intervals are quite large and often do not rule out effects of zero post-implementation.[17]

While I find little evidence of pre-existing trends in education outcomes leading up to state EITC implementation, there may be other state-specific events that occur at the same time as changes in state EITC generosity. For instance, if states are more likely to increase their EITC benefits when the unemployment rate is high and high unemployment also induces more individuals to enroll in college, changes in education outcomes will reflect not only the effect of a change in state EITC benefits, but also the effect of high unemployment on educational attainment. To test whether state EITCs are correlated with other macroeconomic indicators, I regress the maximum federal and state EITC on other state characteristics such as state-by-year GDP, minimum wage, unemployment rate, welfare generosity, and state spending on higher

[17] I have also conducted an event-study analysis using the CPS, which provides a larger sample size and more precision in estimating time trends. An analogous Figure 3 for the CPS can be found in Appendix Figure 1. Results look quite similar to those found in the SIPP, but suggest significant increases in the outcomes of interest in the years following state EITC implementation.

education. Results (shown in Table 4) suggest little correlation between the generosity of state EITC benefits and other state indicators.

IV. Results

Table 5 shows results from models (1) and (2) for each outcome variable of interest. Education outcomes are evaluated as a function of the maximum federal and state EITC in a given year and state. The top panel presents the coefficient β from (1), while the bottom panel presents the coefficient on the interaction term β_3 from (2). The coefficient can be interpreted as the change in the outcome variable of interest associated with a $1,000 increase in the maximum federal and state EITC benefit. All models include state and year fixed effects. The first column shows results with no other demographic controls, while the second column adds demographic characteristics. Standard errors are clustered at the state level, allowing for correlation of the error term for individuals living in the same state. From left to right, the first panel shows results for the 18--23 year olds in the SIPP, the second panel shows results for 18--20 year olds in the SIPP, and the third panel shows results for 18--20 year olds in the CPS.

Results from the top panel indicate that current and past college enrollment rates increased among 18--23 year olds in the high-impact sample as the maximum EITC benefit increased, as did years of schooling, and high school graduation rates. None of these effects is statistically significant without the demographic controls (column 1), but effects on years of schooling, high school graduation, and the likelihood of ever enrolling in college all increase and become statistically significant once demographic controls are included in column 2. After including demographic controls (column 2), 18--23 year olds are 6 percent more likely to be enrolled in college (a 1.5 percentage point increase on a base of 26 percent) and 7 percent more likely to have ever enrolled in college (2.7 percentage point increase on a base of 41 percent). Smaller effects can be seen for years of schooling, which increased by 1 percent (0.11 of a year increase on a base of 11.97 years), and high school completion, which increased by 3 percent (2.3 percentage point gain on a base of 70 percent). The results for college enrollment are smaller than effects found elsewhere in the literature (Dynarski 2003, Abraham and Clark 2006) which suggest that a $1,000 increase in financial aid is associated with a 3 percentage point increase in college enrollment. The results presented here are about half that size, but a 95% confidence interval includes a 4 percentage point increase in current college enrollment among 18--23 year olds following an increase in the EITC of $1,000. Further, my sample focuses on a group that is likely to be eligible for a lot of financial aid and therefore may be less responsive to short-run changes in household income.

Effects are slightly smaller but less precisely estimated for the samples of 18--20 year olds in the SIPP and the CPS. Results from regressions including demographic controls along with state and year fixed effects for the 18--20 year olds suggest that individuals are 1.7 percentage points more likely to be currently enrolled in college and are 2.7 percentage points more likely to have ever enrolled in college. These effects represent a 6-7% increase in college enrollment among the high-impact sample. For the CPS results, I find no significant effects of the EITC on education outcomes, though point estimates are quite similar to those in the SIPP.

Results from the top panel of Table 5 indicate that individuals who live in states with EITCs are more likely to finish high school and go on to college. While these effects could be due to the changes in the EITC, it is also possible that other state-specific changes may be occurring at the same time as changes in the EITC that affect all teens. To control for other state-specific factors, I next include all of the individuals living with at least one parent in the beginning of the survey, regardless of the education of their parents. Individuals who live in households with at least one parent who has any college experience are less likely to receive the EITC and should therefore be less affected by changes in the value of the state EITC. Including these individuals in the analysis controls for other trends that may be occurring in states at the same time as changes in the EITC.

The coefficient presented in the bottom panel of Table 5 is β_3 from (2). It represents the change in the outcome variable of interest associated with a $1,000 increase in the combined federal and state EITC for individuals in the high-impact sample, compared to those in the low-impact sample and to those who never experience a $1,000 increase in the EITC. Including the low-impact sample as a control group for other state-level changes increases the precision of the estimates. Including demographic controls and state and year fixed effects (column 2), 18--23 year olds in the high-impact sample are 0.7 of a percentage point more likely to be enrolled in college (on a base of 26 percent) and are 1.1 percentage points more likely to have ever enrolled in college (on a base of 41 percent). These effects are smaller than those found for the high-impact sample only, indicating that either there are other changes in the states that implement EITCs that affect the education outcomes of all individuals in the state, or that some of the individuals in the low-impact sample may actually be receiving the EITC as well. If that is the case, these estimates can be thought of as a lower-bound, as some of the individuals in the low-impact sample may also benefit from the increases in the EITC. Results from the triple-difference analysis are also quite similar across the three samples. Effects are largest in the CPS sample (though not statistically significantly different from the SIPP), which may reflect sample selection due to the types of individuals likely to be living at home in the CPS. In the CPS, 18--20 year olds are 1.8 percentage points more likely to be enrolled in college (on a base of 26 percent), 1.5 percentage points more likely to have a high school degree (on a base of 58 percent), and 2 percentage points more likely to have ever enrolled in college (on a base of 33 percent). As estimates from equation (2) are more precisely estimated and reflect a lower-bound on the effects of the EITC on educational attainment, I will focus on the triple-difference specification for all following tables.

Turning to estimates for degree completion, Table 6 shows results only for the 18--23 year olds in the SIPP, since almost no one in the 18--20 year old sample has either an associate's degree or a bachelor's degree. While there is no effect of the EITC on the likelihood of completing an associate's degree there is a small, 0.3 of a percentage point increase in the likelihood of completing a bachelor's degree associated with a $1,000 increase in the EITC. While this effect is quite small, it represents a large change in percentage terms. Just 3 percent of the high-impact sample has a bachelor's degree prior to state EITC implementation, so this represents a 10 percent increase in the share of bachelor's degree holders.

IV. A. Timing of income interventions

To address the question of whether the EITC increases college enrollment by providing a conditional cash transfer to college-aged children or through increasing household income when children are young, I next examine how effects of the maximum EITC vary depending on how old the respondent was at the time of state EITC implementation.[18] Because of the variation in timing of state EITC implementation, some individuals are exposed to the EITC for more years of their childhood. Depending on how old the individual was when the state implemented an EITC, some individuals may have benefited from many years of additional EITC income. Figure 4 plots the effects of the maximum EITC by how old the 18--23 year olds in the SIPP were when the EITC was implemented in their state. If the EITC works through improving the schooling of children when young, we would expect to find larger effects for individuals who were younger

[18] Information about cross-state moves are rather limited in the SIPP, so I assume that the current state of residence was constant throughout an individual's life. Very few individuals move across states throughout the course of the SIPP panel.

when the EITC was implemented in their state. If instead, the EITC helps alleviate short-term credit constraints in paying for college, we would expect to find little variation in the effects of the EITC by age.

The coefficient plotted in Figure 4 is the triple interaction of the maximum EITC with an indicator for being in the high-impact sample for each age group illustrated. All individuals who were living in a state that implemented an EITC after they turned 23 were grouped together, and the reference category is individuals living in states that never implemented EITCs. There is a clear, negative association between age at time of implementation and educational attainment. The largest effects can be seen for individuals who were younger than 12 at the time of implementation, and no effects, or negative effects can be seen for individuals who were 23 or older. This implies that individuals who were exposed to a state EITC for more years of their childhood are more likely to have positive education outcomes, indicating that the EITC does work through improving the schooling of children when they are young. While many of the outcomes are still significant and positive for individuals who were 12--17 or 18--22 when their state implemented an EITC, these graphs suggest that effects are larger for the individuals exposed to the EITC for more years of their childhood.[19] In contrast to the large effects found in Manoli and Turner (2014), this analysis provides no evidence that conditional cash transfers for 18--23 year olds increase college enrollment among disadvantaged youth. My analysis exploits a different type of exogenous variation in household income, generated from state EITC expansions, while Manoli and Turner (2014) use a regression discontinuity approach among EITC recipients just above and below the first kink point in the EITC benefit structure. My estimation incorporates both the income effect generated by the increased generosity of state EITC benefits, as well as the substitution effect of increasing the incentive for 19--23 year olds

[19] Appendix Figure 2 shows the same exercise using the CPS sample of 18--20 year olds. Results using the CPS confirm the results from the SIPP and indicate a stronger, negative association between age at EITC implementation and education outcomes.

to remain in school in order to remain dependents on their parents tax forms, both of which should increase college enrollment. Since all of the individuals in the Manoli and Turner (2014) sample receive the EITC, those estimates reflect only the income effect of increasing cash-on-hand around the time of college enrollment decisions.

IV. B. Effects by gender, race, household composition

Table 7 presents differences in results by gender, family structure, and race. Results indicate that effects are larger and more significant among women, children living with a single parent, and non-Hispanic Blacks. While point estimates for men are somewhat similar to those for women, I only obtain significant results for women. Following an increase in the EITC by $1,000, women are 0.8 percentage points more likely to be enrolled in college and 1.5 percentage points more likely to have ever enrolled in college. Similarly, children living with only one parent are 1.6 percentage points more likely to be enrolled in college and 2.5 percentage points more likely to have ever enrolled in college following an increase in the EITC by $1,000. Approximately 75 percent of all EITC claims come from single-headed households, so even though children from single-parent households tend to be at a disadvantage economically, we would expect that changes in the EITC would primarily affect children from single-headed households. Finally, effects are also more significant for black youth. Following an increase in the EITC by $1,000, black individuals are 1.2 percentage points more likely to be enrolled in college and 2.2 percentage points more likely to have ever enrolled in college. [20]

Lastly, if one of the mechanisms driving increases in educational attainment is the added EITC benefit a household would receive if their child remained in school past age 18, we would expect different responses to the EITC based on the number of children living in the household.

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[20] Approximately half of the 18--23 year olds in my sample living with only one parent were eligible for the EITC in the first year of the SIPP compared to 20 percent of those living with both parents. Similarly for black families---approximately 45 percent of black children were eligible for the EITC in the first year of the SIPP sample, compared to 25 percent of children in non-black families.

Prior to the 2009 tax year, households could only claim up to two children for the EITC---more than two children did not increase the benefit amount. After 2008, families could claim up to three children, so that having a third child both increased the replacement rate on earnings, and the maximum potential benefit. Beyond three children, there is no added EITC benefit for additional children. Therefore we would expect to find smaller or no responses in college enrollment among households with four or more children, since keeping a fourth child in college does not increase the value of the household EITC benefit.

In Figure 5, I show how the likelihood of ever enrolling in college varies by the number of children under 24 (including the respondent) living in the household at the start of the SIPP survey. Here, regressions were estimated separately by gender for each household size. Results in Figure 5 show that effects of a $1,000 increase in the maximum EITC on ever enrolling in college are only significant for households with one or two children. There are relatively few households with four or more children, so the estimates are not precise enough to rule out larger effects for households with four or more children compared to one child, but the trend certainly appears to be a declining effect of the EITC on college enrollment for households with more children. This supports the hypothesis
that effects should be concentrated among households with fewer children, since each additional child up to three children garners a higher household EITC benefit but households with more than three children do not receive a higher benefit than households with exactly three children.

IV. C. Robustness Checks

To test whether parental education serves as a good proxy for EITC eligibility, I replicate the analysis presented in the bottom panel of Table 5 using a number of alternative definitions for the high-impact sample. Results are presented in Appendix Table 3. Using actual eligibility for the EITC generates larger effects for years of schooling and high school graduation than using parents' education as the definition of eligibility. While the results in Table 5 showed no significant increase in high school graduation rates, using actual eligibility indicates that individuals are about 1 percentage point more likely to graduate from high school following an increase in the EITC by $1,000. Results for college enrollment are somewhat comparable to those found in Table 5: following a $1,000 increase in the EITC, individuals are about 1 percentage point more likely to have ever enrolled in college.

To address concerns that sample selection may account for the observed effects, I employ two final strategies: first, I include all of the individuals not living with a parent at the start of the survey in the high-impact sample to test whether results are driven by changes in who is observed in the sample as a function of EITC benefit changes. Finally, I also conduct an analysis restricting the sample of 18--23 year olds to those who were younger than 19 at the start of the survey, which represents approximately 88 percent of that sample. Including individuals not living with a parent as part of the high-impact sample produces somewhat larger results. These individuals tend to be older than those observed living with a parent, so it is possible that these individuals were also affected by EITC changes but moved out of their parents' houses before the start of the SIPP survey. Finally, the last column shows results restricting the sample to 18--23 year olds who were younger than 19 at the start of the survey, which captures nearly 90 percent of individuals fitting the age criterion. These results are quite similar to the main results presented in Table 5, although the reduced sample size leads to less precisely-estimated effects. All of these robustness checks present similar results to those reported in Table 5, suggesting that results are not sensitive to the particular definitions of the high-impact sample or sample selection criteria.

V. Conclusion

This paper analyzes the effect of household income on the educational attainment of children from low-educated households. Using variation in both the timing of implementation and generosity of state-level EITC benefits, I have shown that increasing state EITC benefits significantly increases the educational attainment of children who grow up with low-educated parents. After an increase in EITC benefits by $1,000, 18--23 year olds gain about 0.11 years of schooling, are 2.3 percentage points more likely to have completed high school, and are 2.7 percentage points more likely to have ever enrolled in college, compared to children who live in states that do not implement EITCs. Results are qualitatively quite similar among 18--20 year olds from the SIPP and CPS. Controlling for state-level trends in education outcomes using children from highly-educated backgrounds produces somewhat smaller, more precisely-estimated positive effects. 18--23 year olds growing up in low-educated households are 0.7 percentage points more likely to be enrolled in college and are 1.1 percentage points more likely to have ever enrolled in college compared to individuals from highly-educated households. While few individuals in this sample complete a bachelor's degree (3 percent of 18--23 year olds in states that implement EITCs), I find a 10 percent increase in the likelihood of completing a bachelor's degree associated with a $1,000 increase in the EITC. In analyzing differences by subgroups, I find that effects are larger and more significant for women, individuals who grow up in single-parent households, and black children.

In assessing whether the EITC works to improve the education outcomes of children by providing short-run increases to household income during college-going years or through increases in household income when children are young, I find evidence that effects are largest for children who were exposed to a state EITC before age 12. This suggests that the EITC improves the educational attainment of children growing up in low-educated households by increasing household income throughout their lives. This supports previous research examining the test scores of children of EITC-recipients (Chetty et al. 2011, Dahl and Lochner 2012). Building on the results from Dahl and Lochner (2012), Chetty et al. (2011) examine the impact of an increase in child test scores (generated from changes in teacher quality) on college enrollment for a sample of children from a large, urban school district. The authors find that a 1 standard deviation increase in a single year of test scores leads to a 5.5 percentage point increase in college enrollment rates. To put this finding in the context of Dahl and Lochner (2012), the authors calculate that a $1,000 increase in the EITC should lead to a 0.3 percentage point increase in college enrollment. The estimates in this analysis are slightly larger than the 0.3 percentage point increase in college enrollment found by Chetty et al. (2011) but may reflect differences in identification strategy or different potential mechanisms through which the EITC affects college enrollment. Chetty et al. (2011) estimate the effects of the EITC on college enrollment solely through a one-year increase in test scores. My estimates suggest that the EITC may increase college enrollment through more channels than just test scores (e.g. through conditional cash transfers to college-aged children or the development of non-cognitive skills).

This analysis has shown that, in addition to lifting millions of households out of poverty each year, the EITC also helps increase the educational attainment of children from economically-disadvantaged households. The EITC is a wide-reaching program that affects a large share of families in the United States. Recent estimates suggest that fully half of all households with children will claim the EITC at some point over an 18 year period (Horowitz and Dowd 2011). I find evidence that the EITC helps children enroll in college and complete more years of schooling, which supports previous research linking the EITC to higher test scores among low-income children (Dahl and Lochner 2012). This provides further evidence that the EITC not only works to lift families out of poverty for the current generation, but also provides hope of upward mobility for future generations of children from low socioeconomic backgrounds.

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Table 1. States with Earned Income Tax Credits, year of implementation, benefit level at implementation, and in 2011 tax year (as a percent of federal EITC)

	Year of implementation	Benefit level in year of implementation (as a percent of federal benefit)	Benefit level as of 2011 tax year (as a percent of federal benefit)
Rhode Island	1986	22.2	25
Vermont	1988	23	32
Wisconsin[1]	1989	75	34
Iowa	1990	5	7
Minnesota[2]	1991	10	45
New York	1994	7.5	30
Massachusetts	1997	10	15
Oregon	1997	5	6
Kansas	1998	10	18
Maryland	1998	10	25
Colorado	1999	8.5	0
DC	2000	10	40
Illinois	2000	5	5
Maine	2000	5	5
New Jersey	2000	10	20
Oklahoma	2002	5	5
Indiana	2003	6	9
Nebraska	2003	8	10
Delaware	2006	20	20
Virginia	2006	20	20
New Mexico	2007	8	10
North Carolina	2008	3.5	5
Michigan	2008	10	20
Louisiana	2008	3.5	3.5
Connecticut	2011	30	30

Source: Tax Policy Center http://www.taxpolicycenter.org/taxfacts/displayafact.cfm?Docid=293

1: Wisconsin has a system based on the number of children in the household. Rate shown here is for households with 3 or more children.

2: Minnesota has a system based on whether there are any children living in the household, and after 1997, household earnings. Rate shown here is for households with children and the maximum possible rate given income.

Table 2. Descriptive statistics before and after state EITC implementation, relative to individuals who live in states that never implement EITCs, 18-23 year olds living with at least one parent at the beginning of the survey and neither parent has more than a high school degree

	Never-implementing states	Pre-state EITC	Post-state EITC	Change after implementation (std. err.)
Currently enrolled in college	0.23	0.26	0.29	0.047
	(.421)	(.436)	(.452)	(.022)
Years of schooling	11.85	11.97	12.08	0.189
	(1.532)	(1.605)	(1.482)	(.062)
Has a high school degree	0.66	0.70	0.71	0.041
	(.475)	(.459)	(.453)	(.02)
Ever enrolled in college	0.37	0.41	0.43	0.050
	(.481)	(.491)	(.495)	(.023)
Has at least an associate's degree	0.04	0.04	0.07	0.029
	(.203)	(.2)	(.258)	(.009)
Has at least a bachelor's degree	0.02	0.03	0.03	0.013
	(.138)	(.165)	(.184)	(.004)
Black	0.19	0.24	0.15	-0.053
	(.39)	(.425)	(.357)	(.048)
Hispanic	0.305	0.122	0.193	-0.053
	(.46)	(.327)	(.394)	(.084)
Female	0.44	0.43	0.50	0.063
	(.496)	(.496)	(.5)	(.017)
Age	20.11	20.21	20.16	0.013
	(.95)	(1.672)	(1.663)	(.046)
Maximum federal and state EITC (in 1,000s of 2011$)	3.995	3.214	5.793	2.050
	(1.384)	(1.362)	(1.07)	(.284)
Number of male siblings in the household	0.716	0.612	0.639	-0.043
	(.95)	(.827)	(.892)	(.058)
Number of female siblings in the household	0.629	0.532	0.608	0.011
	(.881)	(.813)	(.868)	(.053)
Living with both parents	0.651	0.647	0.622	-0.028
	(.477)	(.478)	(.485)	(.029)
Number of observations	17,271	7,806	5,642	

Source: Survey of Income and Program Participation 1990, 1991, 1992, 1993, 1996, 2001, 2004, 2008, 18-23 year olds living with at least one parent in the first month of the survey, neither parent has schooling beyond a high school degree. Standard deviations in parentheses.

Table 3. Means of outcome variables of interest for individuals living with their parents at the beginning of the survey and neither parent has more than a high school degree, before and after state EITC implementation relative to states that never implement EITCs, by data source

SIPP 18-20 year olds

	Never-implementing states	Pre-state EITC	Post-state EITC	Change after implementation (std. err.)
Currently enrolled in college	0.240	0.281	0.303	0.051
	(.427)	(.449)	(.459)	(.026)
Years of schooling	11.61	11.72	11.77	0.123
	(1.415)	(1.418)	(1.302)	(.071)
Has a high school degree	0.557	0.592	0.614	0.046
	(.497)	(.49)	(.487)	(.024)
Ever enrolled in college	0.312	0.364	0.371	0.043
	(.463)	(.481)	(.483)	(.028)
Number of observations	10,908	4,628	3,489	

CPS 18-20 year olds

	Never-implementing states	Pre-state EITC	Post-state EITC	Change after implementation (std. err.)
Currently enrolled in college	0.229	0.258	0.313	0.076
	(.42)	(.438)	(.464)	(.006)
Years of schooling	11.53	11.61	11.73	0.185
	(1.305)	(1.34)	(1.28)	(.017)
Has a high school degree	0.545	0.580	0.605	0.051
	(.498)	(.494)	(.489)	(.006)
Ever enrolled in college	0.297	0.328	0.382	0.077
	(.457)	(.47)	(.486)	(.006)
Number of observations	19,884	6,860	8,628	

Source: Survey of Income and Program Participation 1990, 1991, 1992, 1993, 1996, 2001, 2004, 2008, 18-20 year olds living with at least one parent in the first month of the survey, neither parent has schooling beyond a high school degree. Current Population Survey March Supplement 1992-2011, sample of 18-20 year olds living with at least one parent at interview, has schooling beyond a high school degree.

Note: Individuals who live in states that never implement EITCs serve as the comparison group. Standard deviations in parentheses.

Table 4. Test for exogeneity of State EITC benefits

Dependent Variable:	States that ever implement EITCs Maximum federal and state EITC in year t (in thousands of 2011 dollars)	All states Maximum federal and state EITC in year t (in thousands of 2011 dollars)
Log state GDP per capita	-0.650	0.258
	(.759)	(.436)
Unemployment rate (*100)	-0.047	-4.90E-04
	(.036)	(.022)
Log real minimum wage	-0.298	-0.101
	(.174)	(.079)
Per student spending on higher education (*1,000)	-0.005	-0.023
	(.046)	(.023)
Maximum monthly welfare benefit for family of 3 (*100)	0.052	0.027
	(.113)	(.053)
State fixed effects	Y	Y
Year fixed effects	Y	Y
Number of observations	480	891

Source: Statistics on state-level unemployment rates, state-level GDP, and state-level minimum wage from 1990-2011. State-level spending on higher education from the State Higher Education Executive Officers. *** p<.01 ** p<.05 * p<.10

Table 5. The effect of the maximum federal and state EITC benefit on educational outcomes of individuals living with at least one parent in the first month of the survey. By data source.

High-impact sample	SIPP 18-23 year olds		SIPP 18-20 year olds		CPS 18-20 year olds	
	No controls	W/controls	No controls	W/controls	No controls	W/controls
Outcome variable						
Currently enrolled in college	0.014	0.015	0.017	0.017	0.010	0.010
	(0.015)	(0.012)	(0.014)	(0.011)	(0.016)	(0.016)
Years of Schooling	0.089	0.107 **	0.013	0.021	0.045	0.040
	(0.075)	(0.051)	(0.064)	(0.050)	(0.049)	(0.052)
High school graduate	0.016	0.023 *	0.009	0.007	0.010	0.007
	(0.021)	(0.012)	(0.022)	(0.016)	(0.016)	(0.016)
Ever enrolled in college	0.022	0.027 **	0.027	0.027 *	0.024	0.023
	(0.017)	(0.012)	(0.018)	(0.015)	(0.020)	(0.019)
Demographic controls	N	Y	N	Y	N	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y
Number of observations	31,130	31,130	19,285	19,285	36,063	36,063

Triple-difference	SIPP 18-23 year olds				SIPP 18-20 year olds				CPS 18-20 year olds			
	No controls		W/controls		No controls		W/controls		No controls		W/controls	
Outcome variable												
Currently enrolled in college	0.006	*	0.007	**	0.009	**	0.010	***	0.018	***	0.018	***
	(.003)		(.003)		(.003)		(.003)		(.003)		(.003)	
Years of Schooling	0.020		0.026	*	0.017		0.021	*	0.050	***	0.049	***
	(.015)		(.014)		(.012)		(.011)		(.011)		(.01)	
High school graduate	0.001		0.003		0.004		0.005	*	0.016	***	0.015	***
	(.004)		(.003)		(.003)		(.003)		(.005)		(.004)	
Ever enrolled in college	0.010	***	0.011	***	0.009	**	0.010	***	0.019	***	0.019	***
	(.003)		(.003)		(.003)		(.003)		(.003)		(.002)	
Demographic controls	N		Y		N		Y		N		Y	
Year fixed effects	Y		Y		Y		Y		Y		Y	
State fixed effects	Y		Y		Y		Y		Y		Y	
Number of observations	81,724		81,724		51,374		51,374		97,123		97,123	

Source: Survey of Income and Program Participation 1990, 1991, 1992, 1993, 1996, 2001, 2004, 2008, 18-23 year olds living with at least one parent in the first month of the survey. Current Population Survey March Supplement 1992-2011, sample of 18-20 year olds living with at least one parent at interview.

Note: OLS Models, clustered standard errors at state level presented in parentheses. Separate regressions for each cell. SIPP outcomes evaluated in each March of the survey. Coefficient in top panel is maximum federal and state EITC value in a given year (in thousands of dollars, 2011$), coefficient in bottom panel is maximum federal and state EITC value in a given year interacted with indicator for whether the individual lives in a household where neither parent has more than a high school degree. Demographic controls include gender, race, number of male and female siblings living in the household, whether both parents are present in the household, a control for number of times the respondent appears in the sample (up to 4 times, SIPP sample only), state-level unemployment rate, and minimum wage. *** indicates significance at p<.01, ** p<.05, * p<.10.

Table 6. The effect of the maximum federal and state EITC benefit on degree attainment of 18-23 year olds living with their parents in the first month of the survey.

Triple-difference	SIPP 18-23 year olds			
	No controls		Add controls	
Outcome variable				
Has at least an associate's degree	-0.002		-0.001	
	(0.002)		(0.002)	
Has at least a bachelor's degree	0.003	*	0.003	*
	(0.001)		(0.002)	
Demographic controls	N		Y	
Year fixed effects	Y		Y	
State fixed effects	Y		Y	
Number of observations	81,724		81,724	

Source: Survey of Income and Program Participation 1990, 1991, 1992, 1993, 1996, 2001, 2004, 2008, 18-23 year old children living with at least one parent in the first month of the survey.

Note: OLS Models, clustered standard errors at state level presented in parentheses. Separate regressions for each cell. SIPP outcomes evaluated in each March of the survey.Coefficient is maximum federal and state EITC value in a given year interacted with indicator for whether the individual lives in a household where neither parent has more than a high school degree. Demographic controls include gender, race, number of male and female siblings living in the household, whether both parents are present in the household, a control for number of times the respondent appears in the sample (up to 4 times), state-level unemployment rate, and minimum wage. *** indicates significance at p<.01, ** p<.05, * p<.10.

Table 7. The effect of the maximum federal and state EITC benefit on educational outcomes of 18-23 year olds living with at least one parent in the first month of the SIPP survey. Differences by gender, family structure, and race

	Differences by gender		Differences by family structure		Differences by race		
	Men	Women	Both parents	Single parent	White	Black	Hispanic
Outcome variable							
Currently enrolled in college	0.006	0.008 *	0.002	0.016 ***	0.004	0.012 *	0.005
	(.005)	(.004)	(0.003)	(0.006)	(0.003)	(0.007)	(0.009)
Years of Schooling	0.028	0.023	0.014	0.046	0.016	0.029	0.037
	(.017)	(.017)	(0.016)	(0.028)	(0.019)	(0.028)	(0.031)
High school graduate	0.004	0.001	0.002	0.006	0.001	0.001	0.003
	(.004)	(.004)	(0.004)	(0.006)	(0.004)	(0.007)	(0.008)
Ever enrolled in college	0.008	0.015 ***	0.006	0.024 ***	0.007	0.022 **	-0.002
	(.005)	(.004)	(0.004)	(0.006)	(0.005)	(0.010)	(0.009)
Demographic controls	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y	Y
Number of observations	43,280	38,444	58,740	22,984	53,395	10,612	10,158

Source: Survey of Income and Program Participation 1990, 1991, 1992, 1993, 1996, 2001, 2004, 2008, 18-23 year olds living with at least one parent in the first month of the survey.

Note: OLS Models, clustered standard errors at state level presented in parentheses. Separate regressions for each cell. Outcomes from the SIPP evaluated in each March of the survey. Coefficient is maximum federal and state EITC value in a given year (in thousands of dollars, 2011$) interacted with indicator for living in a household where neither parent has schooling beyond a high school degree. Demographic controls include gender, race, number of male and female siblings living in the household, whether both parents are present in the household, a control for number of times the respondent appears in the sample (up to 4 times, SIPP only), state-level unemployment rate, and minimum wage. *** indicates significance at p<.01, ** p<.05, * p<.10.

Figure 1. Maximum federal and state EITC by time to state EITC implementation, in thousands of dollars (2011$)

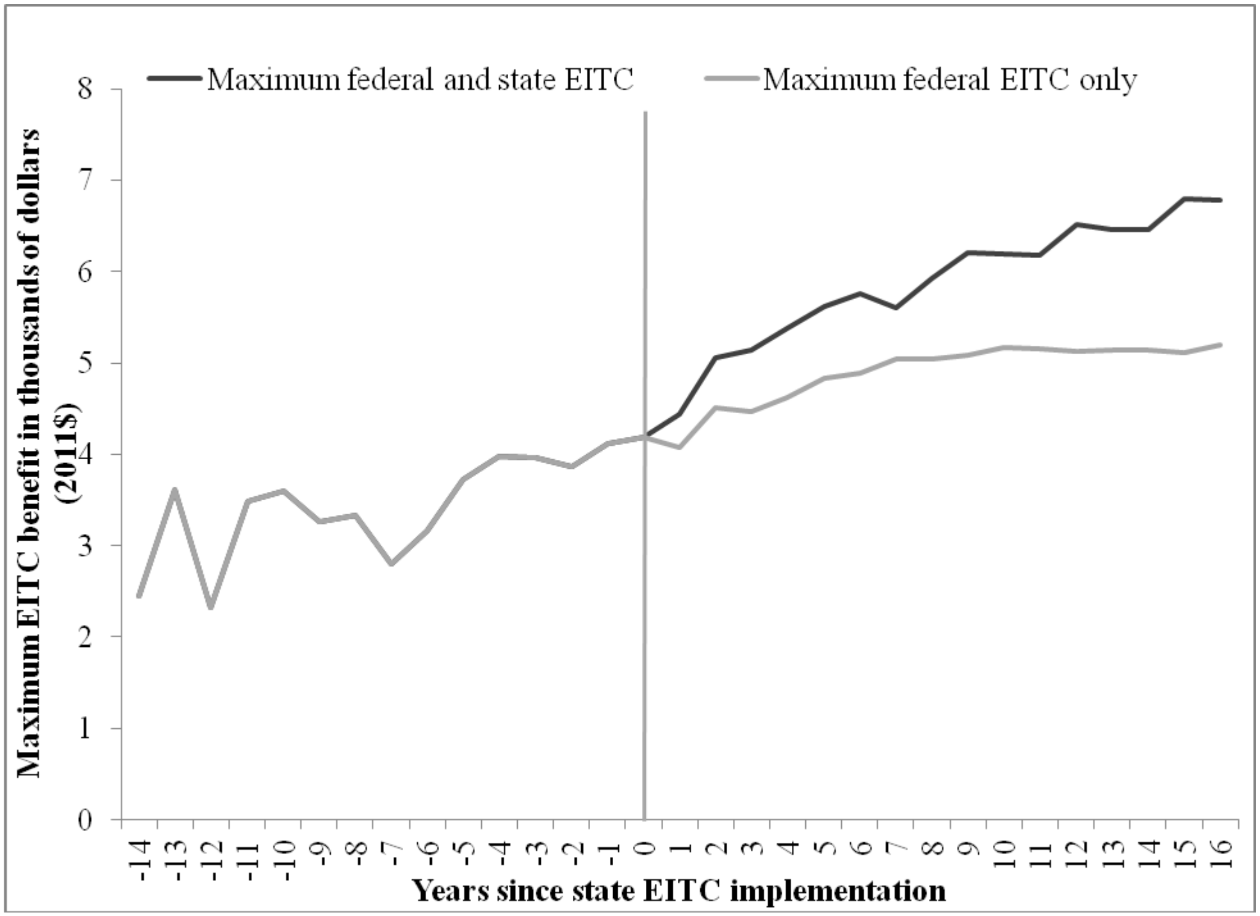


Figure 2. EITC tax schedule for 2011, single-headed households by number of children and earnings

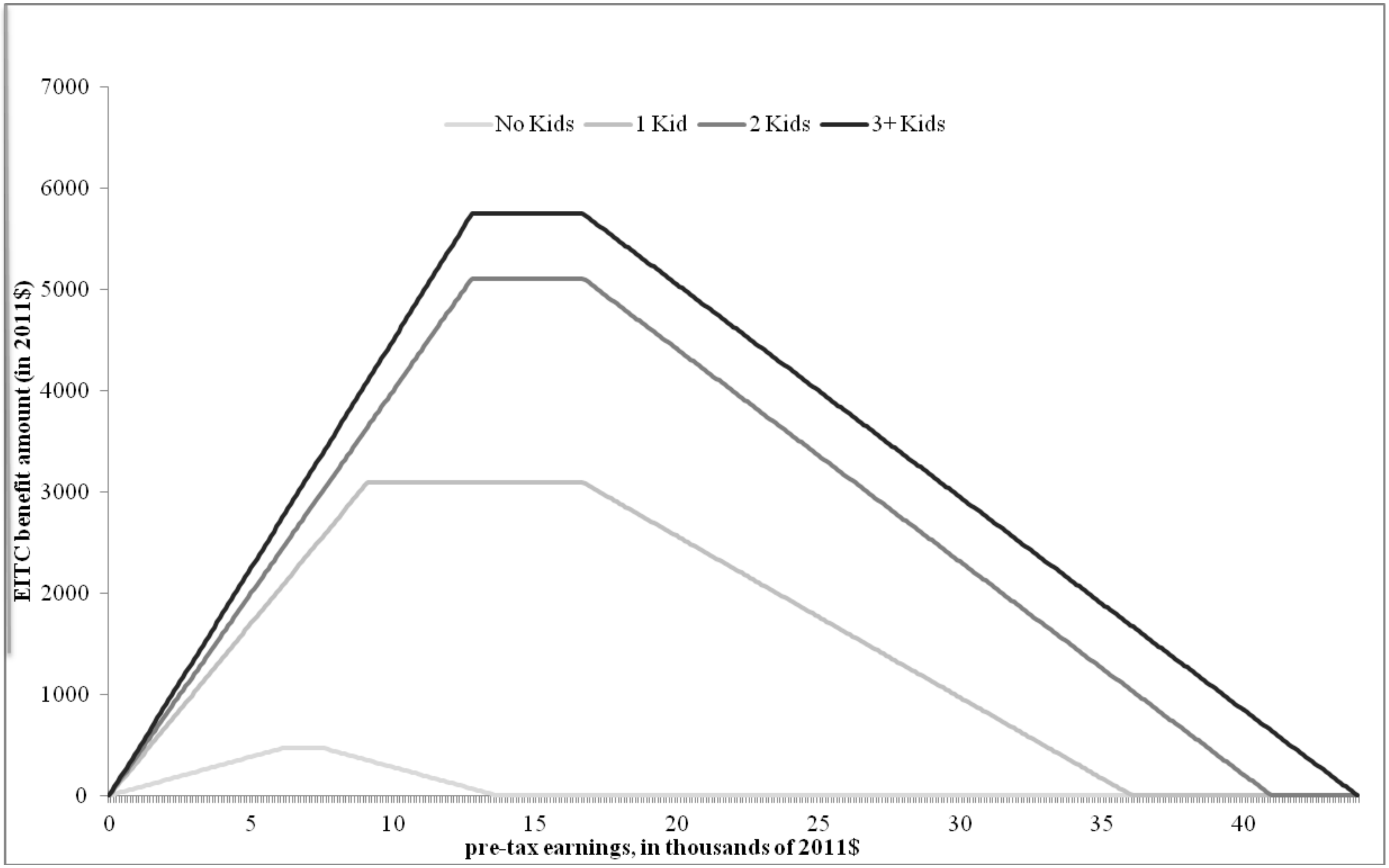
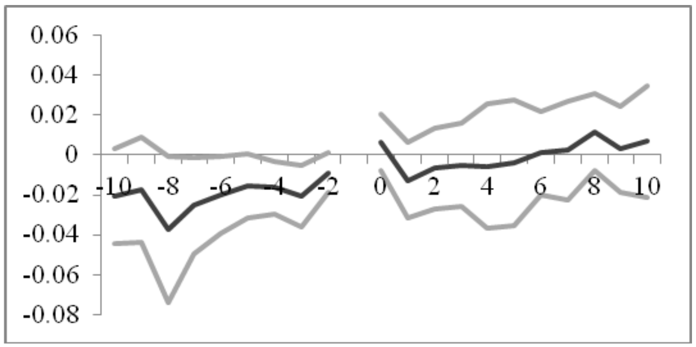
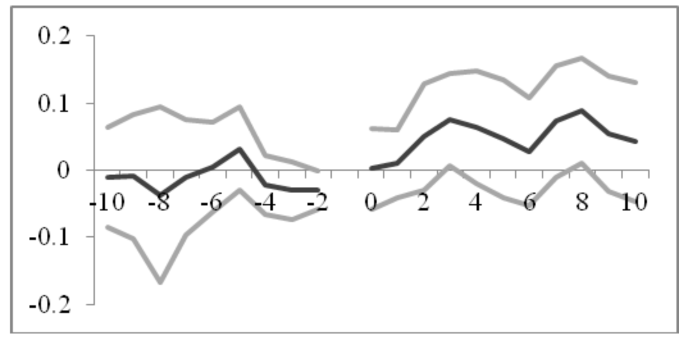


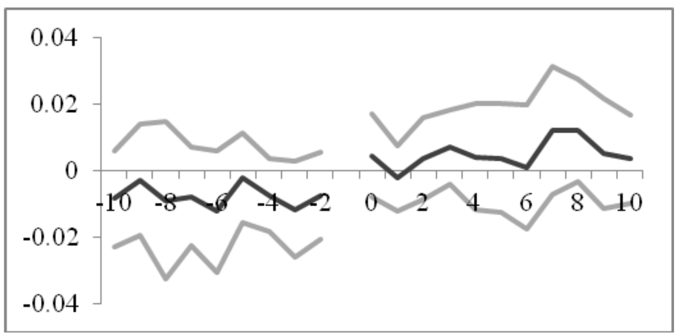
Figure 3. Effect of EITC on outcomes, by time since state EITC implementation; 10 years before and after implementation, relative to states that never implement; 18-23 year olds



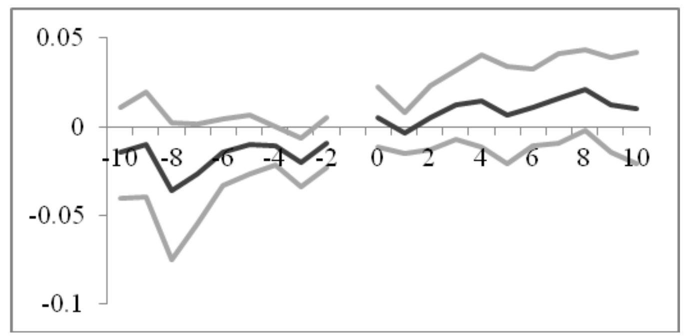
a) Likelihood of being enrolled as full-time college student



b) Number of years of schooling



c) Likelihood of having a high school degree

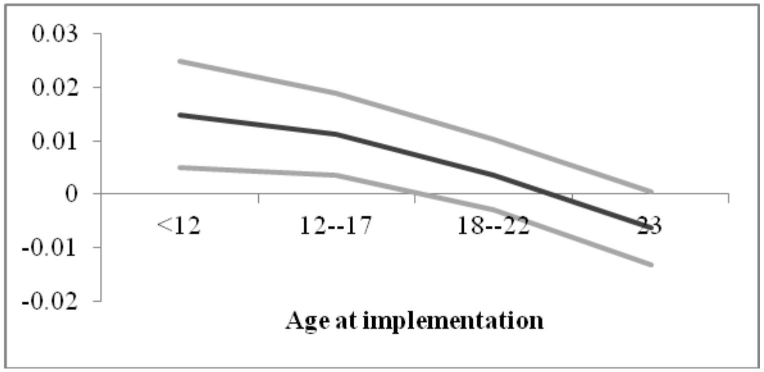


d) Likelihood of ever enrolling in college

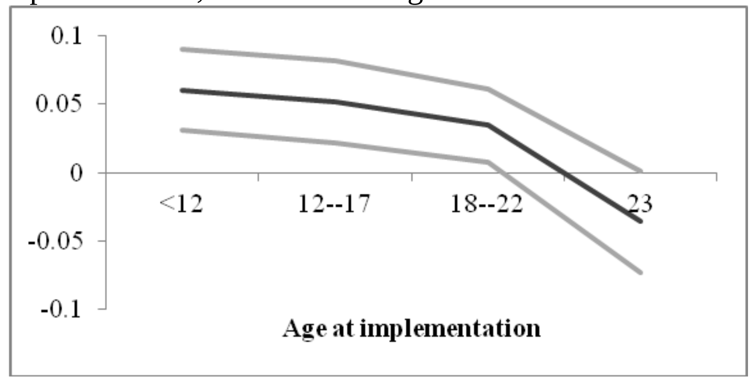
Source: Survey of Income and Program Participation 1990, 1991, 1992, 1993, 1996, 2001, 2004, and 2008.

Note: Graph of the effect of the maximum EITC interacted with an indicator for being in the high impact sample on the outcome indicated below each graph for individuals by years until (since) state EITC implementation, relative to states that never implement EITCs. Effects normalized to zero in the year before implementation. Graphs generated from linear regressions that include basic demographic characteristics: race, age, gender, number of siblings living in the household, an indicator for whether both parents live in the household, and number of times included in the sample. State unemployment rate and minimum wage also included. State and year fixed effects included in all models, standard errors clustered at the state level. 95% confidence intervals indicated with lighter grey lines.

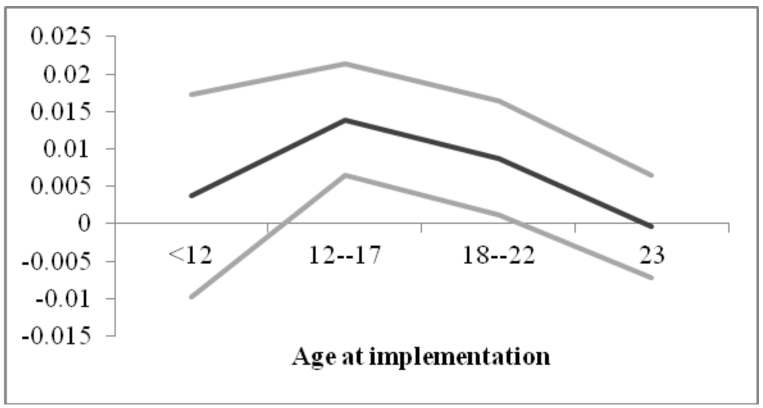
Figure 4. Effect of the EITC on outcomes of interest by age at state EITC implementation, reference: living in a state with no EITC



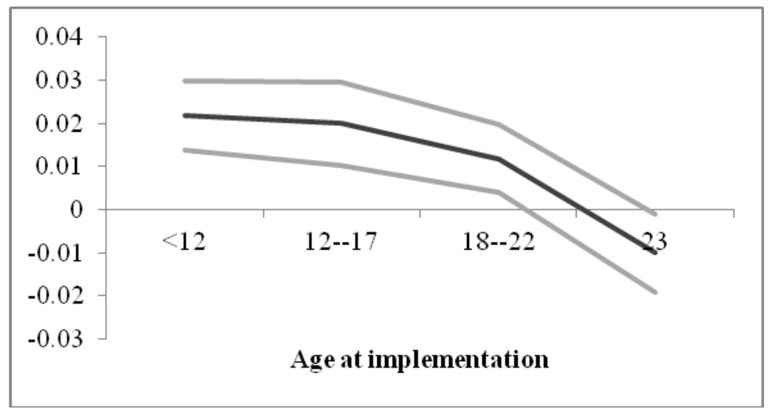
a) Likelihood of being enrolled as full-time college student



b) Number of years of schooling



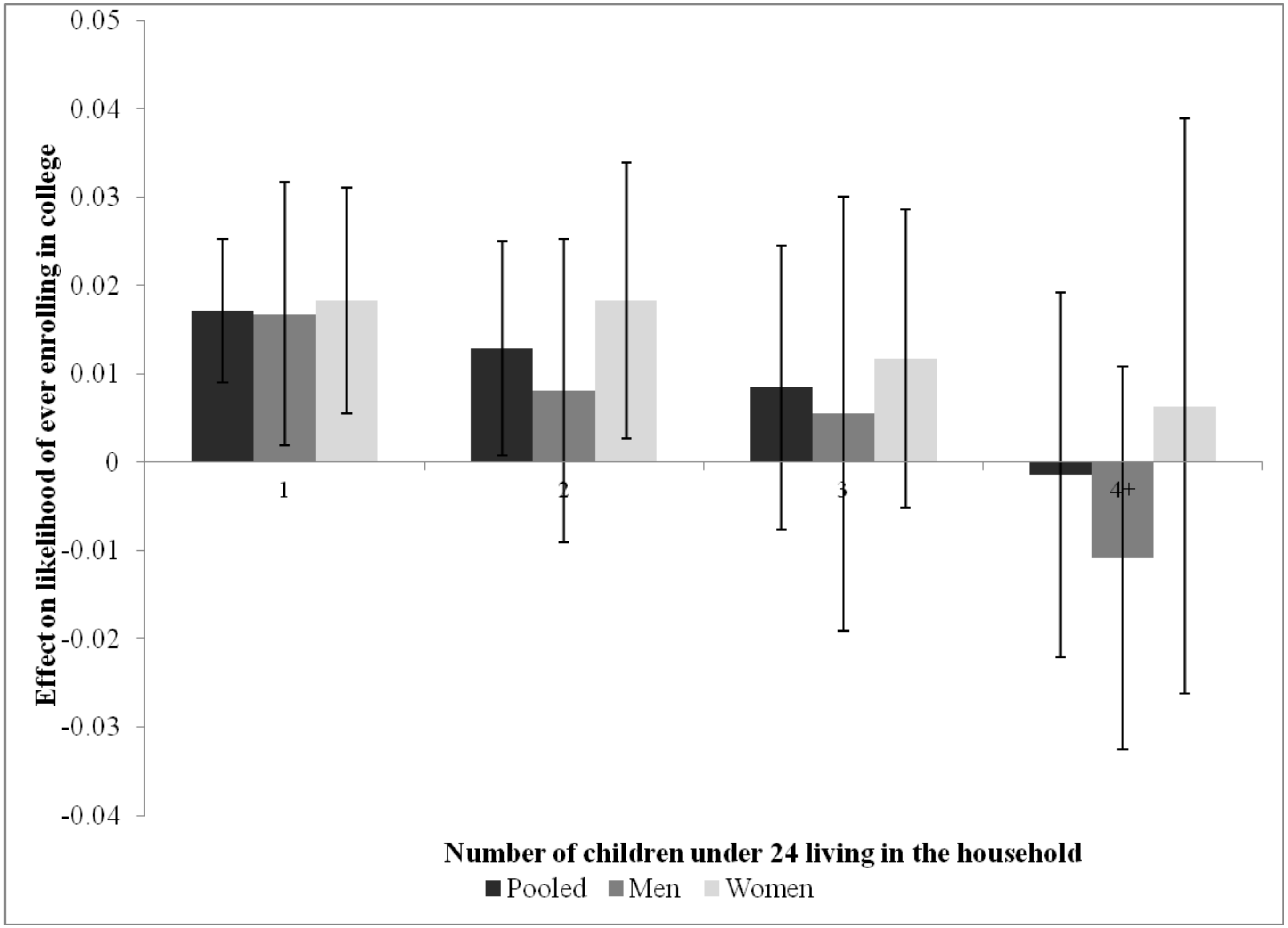
c) Likelihood of having a high school degree



d) Likelihood of ever enrolling in college

Source: Survey of Income and Program Participation 1990, 1991, 1992, 1993, 1996, 2001, 2004, and 2008.
Note: Graph of the effect of the maximum EITC interacted with an indicator for being in the high-impact sample on the outcome indicated below each graph for individuals at each age category. Age categories represent how old the respondent was when the state implemented the EITC. Reference age group is individuals living in states that never implement EITCs.

Figure 5. Effect of maximum EITC in a given state and year on likelihood of ever enrolling in college for high-impact sample relative to low-impact sample, by number of children under age 24 living in the household, 18-23 year olds in the SIPP



Appendix Table 1. Descriptive statistics by whether the individuals resides with at least one parent at the time of interview, by data source

	SIPP 18-23 year olds		SIPP 18-20 year olds		CPS 18-20 year olds	
	Living with at least one parent	Not living with a parent	Living with at least one parent	Not living with a parent	Living with at least one parent	Not living with a parent
Currently enrolled in college	40.5%	18.3%	42.3%	21.9%	40.7%	23.9%
	(.491)	(.386)	(.002)	(.414)	(.002)	(.002)
Years of Schooling	12.38	12.20	12.04	11.64	11.94	11.64
	(1.512)	(2.003)	(.006)	(1.735)	(.004)	(.008)
Has a high school degree	76.5%	77.0%	67.2%	64.6%	65.5%	66.5%
	(.424)	(.421)	(.002)	(.478)	(.002)	(.002)
Ever enrolled in college	57.3%	45.7%	50.8%	35.2%	49.0%	35.5%
	(.495)	(.498)	(.002)	(.478)	(.002)	(.002)
Has at least an Associate's degree	8.5%	12.6%	1.6%	2.8%	1.2%	2.1%
	(.278)	(.332)	(.001)	(.164)	(.)	(.001)
Has at least a Bachelor's degree	5.1%	7.6%	0.2%	0.5%	0.2%	0.4%
	(.22)	(.265)	(.)	(.067)	(.)	(.)
Black	14.5%	13.6%	14.4%	16.6%	13.9%	18.6%
	(.352)	(.343)	(.002)	(.372)	(.001)	(.002)
Other	6.0%	5.4%	6.2%	5.9%	6.1%	7.0%
	(.238)	(.226)	(.001)	(.235)	(.001)	(.001)
Female	45.9%	58.7%	47.3%	59.4%	45.7%	57.9%
	(.498)	(.492)	(.002)	(.491)	(.002)	(.002)
Age	20.09	21.33	18.94	19.25	18.88	19.21
	(1.646)	(1.546)	(.004)	(.795)	(.003)	(.004)
Number of Observations	81,724	37,759	51,374	11,951	97,123	42,260

Source: Survey of Income and Program Participation 1990, 1991, 1992, 1993, 1996, 2001, 2004, 2008, 18-23 year olds. Current Population Survey March Supplement 1992-2011, 18-20 year olds. Current Population Survey March Supplement 1992-2011, sample of 18-20 year olds living with at least one parent at interview.

Appendix Table 2. Effect of the maximum federal and state EITC on demographic controls; individuals living with at least one parent at the beginning of the survey

	High-impact sample only						Triple-difference					
	18-23 year olds, SIPP		18-20 year olds, SIPP		18-20 year olds, CPS		18-23 year olds, SIPP		18-20 year olds, SIPP		18-20 year olds, CPS	
Outcome variable												
Black	0.016		0.011		0.006		-0.003		-0.004		0.003	
	(.024)		(.02)		(.011)		(.004)		(.004)		(.003)	
Female	0.009		0.007		0.003		0.003		0.002		0.002	
	(.015)		(.019)		(.014)		(.003)		(.005)		(.004)	
Age	-0.037		0.032	*	0.017		-0.006		-0.001		0.005	
	(.034)		(.017)		(.026)		(.008)		(.005)		(.004)	
Number of male siblings	0.083		0.106		-0.008		0.004		0.000		0.004	
	(.065)		(.079)		(.032)		(.011)		(.012)		(.008)	
Number of female siblings	0.015		0.030		-0.008		0.012		0.016		-0.004	
	(.041)		(.042)		(.036)		(.011)		(.012)		(.011)	
Living with both parents	0.015		0.005		-0.002		-0.014	***	-0.015	***	-0.009	***
	(.018)		(.018)		(.018)		(.004)		(.004)		(.003)	
State fixed effects	Y		Y		Y		Y		Y		Y	
Year fixed effects	Y		Y		Y		Y		Y		Y	
Number of Observations	31,130		19,285		36,063		81,724		51,374		97,123	

Source: Survey of Income and Program Participation 1990, 1991, 1992, 1993, 1996, 2001, 2004, 2008, 18-23 year olds living with at least one parent in the first month of the survey. Current Population Survey March Supplement 1992-2011, sample of 18-20 year olds living with at least one parent at interview.

Note: OLS Models, clustered standard errors at state level presented in parentheses. Separate regressions for each cell. SIPP outcomes evaluated in each March of the survey. Coefficient is maximum federal and state EITC value in a given year interacted with indicator for whether the individual lives in a household where neither parent has more than a high school degree. *** indicates significance at p<.01, ** p<.05, * p<.10.

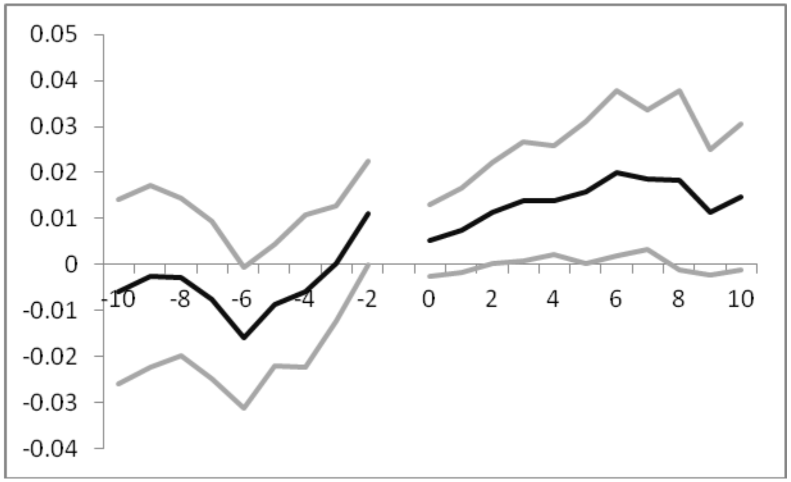
Appendix Table 3. The effect of the maximum federal and state EITC benefit on educational outcomes of 18-23 year olds, alternate specifications of sample

	Main results from table 5		Actual eligibility in first year of survey		Include individuals not living with a parent in treated sample		Restrict to individuals under age 19 at start of survey	
Outcome variable								
Currently enrolled in college	0.007	**	0.004		0.014	***	0.006	
	(.003)		(.003)		(.002)		(.004)	
Years of Schooling	0.026	*	0.043	***	0.051	***	0.022	
	(.014)		(.011)		(.014)		(.013)	
High school graduate	0.003		0.012	***	0.006	**	0.007	*
	(.003)		(.003)		(.002)		(.004)	
Ever enrolled in college	0.011	***	0.013	***	0.023	***	0.007	**
	(.003)		(.005)		(.002)		(.004)	
Demographic Controls	Y		Y		Y		Y	
Year Fixed Effects	Y		Y		Y		Y	
State Fixed Effects	Y		Y		Y		Y	
Number of Observations	81,724		81,724		119,483		42,947	

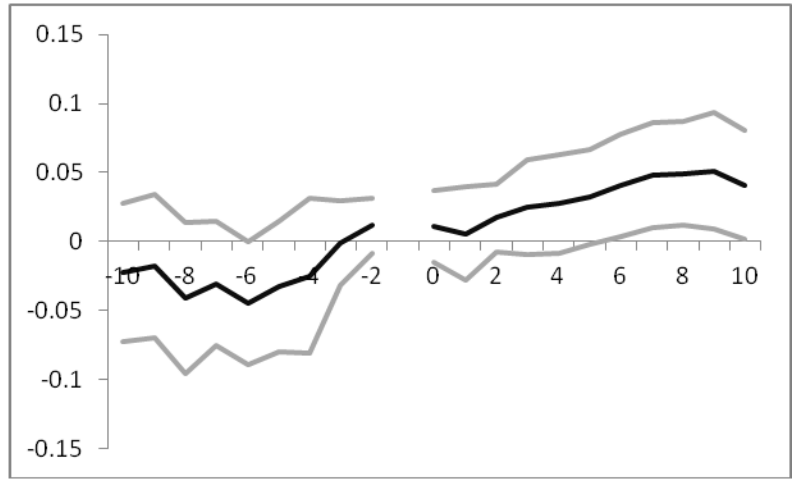
Source: Survey of Income and Program Participation 1990, 1991, 1992, 1993, 1996, 2001, 2004, 2008, 18-23 year old children living with at least one parent in the first month of the survey.

Note: OLS Models, clustered standard errors at state level presented in parentheses. Standard errors in parentheses. Separate regressions for each cell. SIPP outcomes evaluated in each March of the survey. Coefficient is maximum federal and state EITC value in a given year interacted with indicator for whether the household was eligible for the EITC in the first year of the survey. Demographic controls include gender, race, number of male and female siblings living in the household, whether both parents are present in the household, a control for number of times the respondent appears in the sample (up to 4 times), state-level unemployment rate and minimum wage. *** indicates significance at p<.01, ** p<.05, * p<.10.

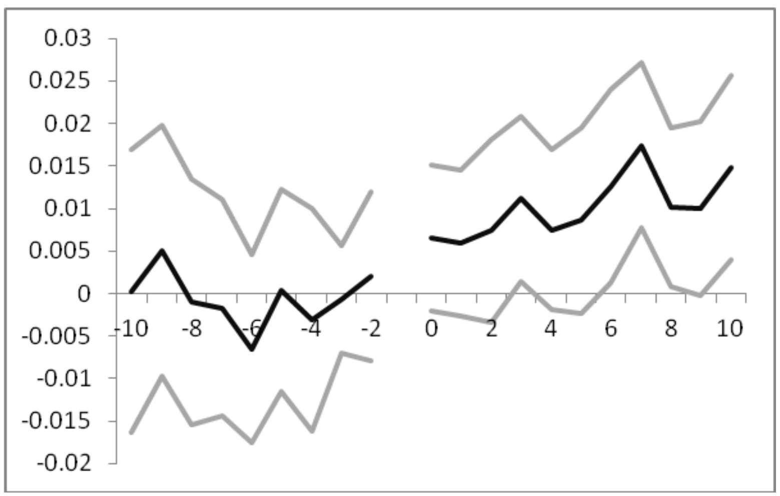
Appendix Figure 1. Effect of EITC on outcomes, by time since state EITC implementation; relative to states that never implement; 18-20 year olds in the CPS (triple-difference)



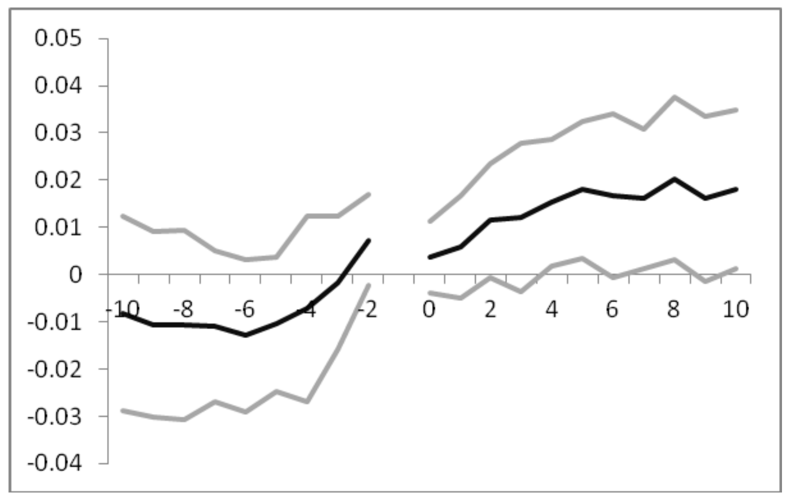
a) Likelihood of being enrolled as full-time college student



b) Number of years of schooling

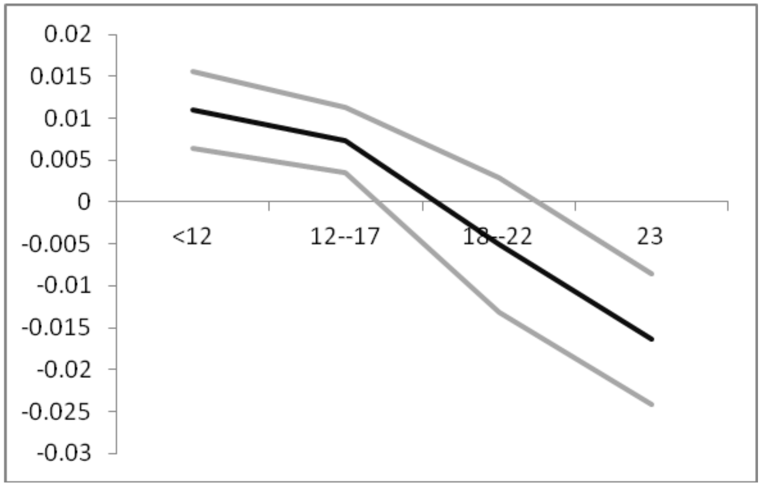


c) Likelihood of having a high school degree

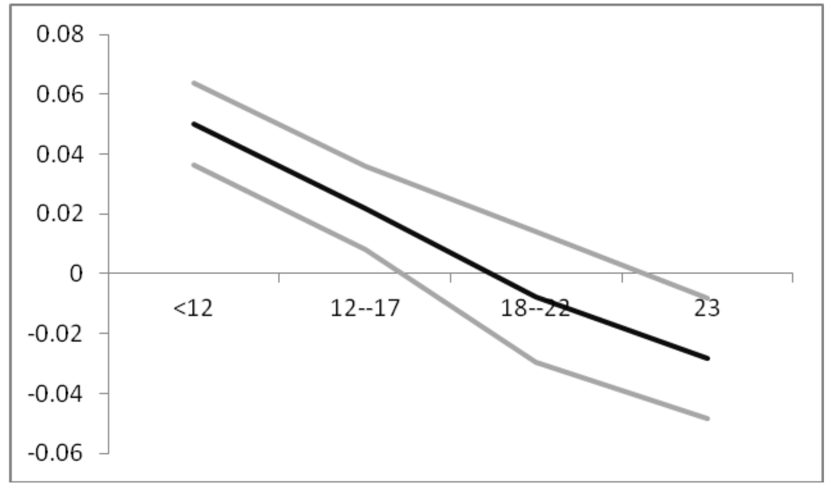


d) Likelihood of ever enrolling in college

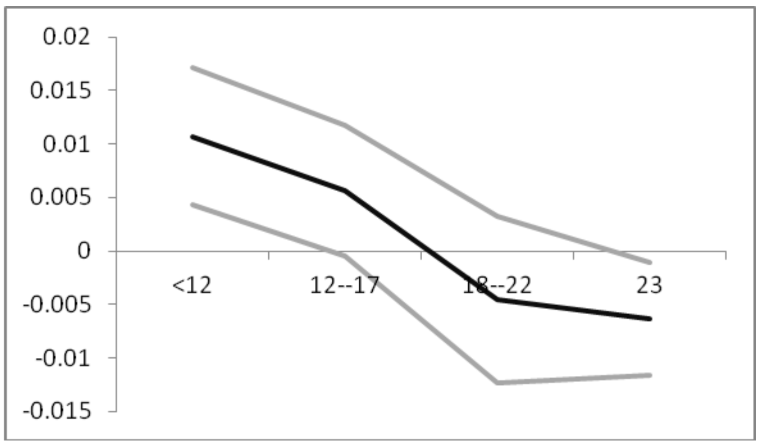
Appendix Figure 2. Effect of the EITC on outcomes of interest by age at state EITC implementation, grouped into five categories: less than 12 years old, 12-17 years old, 18-22 years old, 23 or older, or living in a state that never implements an EITC in the observation window. 18-20 year olds in the CPS, triple-difference sample



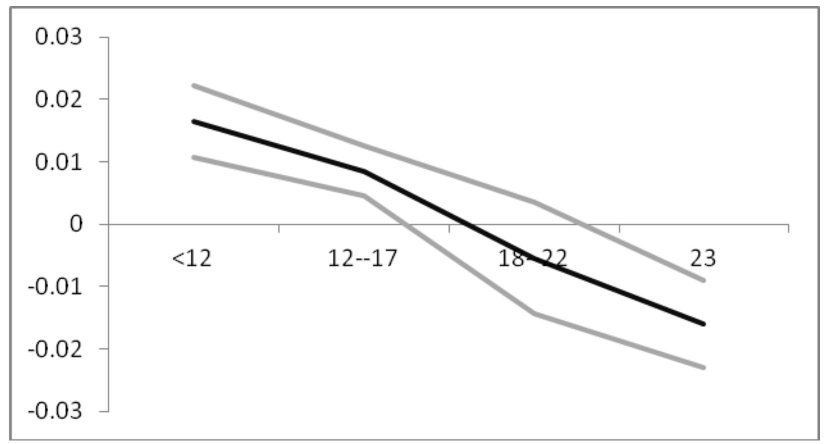
a) Likelihood of being enrolled as full-time college student



b) Number of years of schooling



c) Likelihood of having a high school degree



d) Likelihood of ever enrolling in college

CHAPTER TWO

THE EITC AND FAMILY STRUCTURE: THE IMPACT OF EXPECTED SPOUSE EARNINGS

The earned income tax credit (EITC) has become the largest cash transfer program in the United States, distributing nearly $60 billion dollars in credits in 2010 (Tax Policy Center, 2013). The program has undergone several expansions and revisions since its inception in 1975, not only in absolute benefit level but also in the number of children included in the credit calculation and the income thresholds for married couples. In 2012, the maximum benefit for a household with three children was $5,891. Cohabitation rates have also increased sharply over the last couple of decades and have become less and less associated with marriage, particularly among the low-income population (Bumpass and Lu 2000; Kennedy and Bumpass 2011; Lichter, Qian, and Mellott 2006). The rise in cohabitation has also led to a larger share of births occurring outside of marriage (Martin et al 2011), raising much concern over the well being of children growing up in non-marital households (Brown 2004; Bumpass and Lu 2000; Fomby and Cherlin 2007; Manning and Lichter 1996). The expansion of the EITC over time may have played a role in these trends. Since the EITC is based on family earnings, it may discourage marriage for many dual-earner households, while encouraging traditional, single-breadwinner families.[21] In addition, the trapezoidal structure of the EITC benefit schedule may also create different incentives or disincentives to marry, depending on the level of household earnings. While efforts have been made in recent years to eliminate the marriage penalty from the EITC by increasing the earnings thresholds for married couples, the current policy retains elements that create distortive incentives for marriage.

[21] Recent studies have found that marginal tax rates for a second earner approach nearly 70 percent, once accounting for the phase out of the EITC and other means-tested programs such as food stamps (Kearney and Turner 2013).

Much of the early research on the EITC focused on the labor supply effects (e.g. Eissa and Hoynes 2006, Ellwood 2000, Meyer and Rosenbaum 2001), generally finding a steep increase in the labor supply of single mothers due to the expansion of the EITC. In more recent years, the literature has expanded to analyze the non-financial impacts of the EITC such as child well-being (Dahl and Lochner 2012), health insurance coverage (Baughman 2012), and consumption patterns (Smeeding, Ross-Phillips, and O'Connor 2002; Tach and Halpern-Meekin 2013). Others have studied the impact of the EITC on marriage and divorce (Dickert-Conlin and Houser 2002; Ellwood 2000; Herbst 2011), generally finding small or no effects. Aside from a small supplementary analysis on cohabiting couples and recently married couples in Ellwood (2000), none of the previous work accounts for spouse earnings and expected EITC changes upon marriage for those not already cohabiting or married. Because of the benefit structure, the EITC may encourage marriage for some individuals, but discourage marriage for others; evaluating these different incentives is difficult without considering the impact of spouse earnings.

There has been a considerable amount of work looking at how welfare benefits affect marriage decisions (see Moffitt 1998 for a review), and how the tax structure, more broadly, incentivizes or discourages marriage (e.g. Alm, Dickert-Conlin, and Whittington 1999; Alm and Whittington 2003). Many of these papers find either no effect or only modest impacts of welfare benefits and tax penalties on marriage. Because the EITC provides much larger transfers than the welfare system, we might expect larger disincentives created by the EITC compared to those generated by welfare. Further, unlike the traditional welfare program in the United States, Temporary Aid to Needy Families (TANF), the EITC does not impose lifetime limits to receiving benefits and may therefore have much larger financial consequences to marriage than TANF. The take-up rate for the EITC is also much higher than that of TANF—around 80% for those eligible in 2008, compared to around 40% for TANF (Tax Policy Center, 2011).

In this analysis, I investigate how the EITC has impacted marriage and cohabitation patterns among low-income families, making the following contributions to the literature. First, this analysis adds to the literature on the impact of the EITC on family outcomes by quantifying the expected gains and losses in EITC benefits upon marriage. This paper is the first in the EITC literature to estimate a potential loss or gain in EITC benefits for individuals not currently cohabiting or married, providing the first descriptive picture of the distribution of expected gains or losses in EITC benefits associated with marriage. There are likely heterogeneous treatment effects of the EITC on marriage and cohabitation decisions depending on the expected change in EITC benefits upon marriage. Individuals who expect to gain benefits from the EITC through marriage should subsequently be more likely to marry, while individuals who expect to lose benefits upon marriage should be less likely to marry. Previous studies looking at the EITC and marriage have not considered the impact of spouse earnings when estimating the effect of the EITC on marriage, instead focusing only on the dollar value of the EITC. This analysis expands on prior work by simulating a marriage market to estimate potential gains or losses in EITC benefits associated with marriage for all single women. Finally, while there has been some research looking at the marriage disincentives associated with the EITC, this paper expands on that work to include an analysis of the impact of the EITC on cohabitation decisions. Cohabitation rates have increased sharply over the last few decades and are becoming an increasingly common family structure, particularly for low-income households (Kennedy and Bumpass 2011). In this analysis, I simulate what marriage and cohabitation rates would have been had there been no marriage penalty associated with the EITC.

To conduct this analysis, I first predict the earnings of potential spouses using data on single men from the 2001, 2004, and 2008 Survey of Income and Program Participation (SIPP) in order to calculate an expected loss in EITC benefits upon marriage. Matching single men and women in the SIPP, I then use multinomial logistic regressions to analyze the effect of an expected loss or gain in EITC benefits upon marriage, on decisions for single women to cohabit or marry by the end of the SIPP survey period. Results from the simulated marriage market suggest that the average EITC-eligible woman can expect to lose approximately $1,000 in EITC benefits upon marriage, about 45% of her pre-marriage EITC benefits. From the multinomial logistic regression models, I find that every $1,000 in expected loss in EITC benefits is associated with a 1.8 percentage point decrease in the likelihood of marrying and 1.1 percentage point increase in the probability of cohabiting by the end of the SIPP survey. These effects represent a 10-12% change in marriage and cohabitation rates for this sample of women in the SIPP.

The rest of the paper is structured as follows: in Section 2, I discuss the details of the EITC; in Section 3 I review the current literature on the EITC and family structure. Section 4 discusses the relevant data; Section 5 discusses the simulated marriage market and empirical strategy. Section 6 presents results; Section 7 concludes.

II. Background on the EITC

The EITC benefit structure is made up of three segments—a phase-in region, plateau, and phase-out region. For a household with two children in the phase-in region, every dollar of earned income increases the EITC benefit by 40 cents. Once earnings reach a certain threshold, benefits remain constant until earned income reaches a second threshold, at which point benefits are taxed at a 20% phase-out rate. A similar pattern exists for households with one child or no children, but the phase-in and phase-out tax rates are lower. In addition, 24 states provide supplemental EITC benefits that are calculated based on the federal EITC. These state EITCs are generally calculated as a fixed percentage of the federal EITC, ranging from 3-45% of the federal EITC.

Figure 1 illustrates the federal EITC benefit structure for the 2010 tax year. The solid lines indicate the benefit structure for a single tax payer, while the dotted lines illustrate the structure for a married couple. Beginning in 2002, the plateau region of the benefit structure was extended for married couples in an effort to reduce the marriage penalty associated with the EITC. In 2002, the plateau region was extended for an extra $1,000 for married couples and by 2010, married couples could earn an extra $5,000 before the phase-out took effect. This change in the benefit structure for married couples provides some variation for this analysis since there was no marriage allowance for the first year of observation in the 2001 SIPP, a $1,000 allowance for the first year of the 2004 SIPP, and a $5,000 allowance for the first year of the 2008 SIPP.

The trapezoidal structure of the EITC benefit schedule creates incentives for individuals on the phase-in portion of the schedule to increase their earnings because each dollar of earnings is associated with a larger EITC benefit, while those in the phase-out region may have incentives to reduce their earnings as each additional dollar of earnings is associated with a decline in EITC benefits. For individuals on the plateau, small changes in earnings will not affect EITC benefits. The methods for manipulating earnings may occur either through direct labor market behavior, such as increasing or reducing hours worked, or through marriage decisions.

Working single mothers may have an incentive to remain single if their potential spouses' earnings would reduce EITC benefits or render them ineligible entirely. In contrast, non-working single mothers may have increased incentives to marry working partners in order to receive benefits. To illustrate, a single mother with two children earning $14,000 in 2010 is eligible for the maximum EITC benefit of $5,036. A single, childless man earning $14,000 is not eligible for the EITC. If she marries this single man, bringing their total family income to $28,000, their household benefit falls to $3,656—a loss of $1,379. Under this scenario, the couple might choose to remain unmarried in order to collect the higher benefit and still share income. It is worth noting here that this same hypothetical couple would be penalized to a much greater extent under the 2001 laws than the 2010 laws. In 2001, the benefit structure for a single head of household was the same for a married couple. This same single mother in 2001 would have earned an EITC of about $4,700 (2010$) if she remained single, but her EITC would fall to $1,060 were she to marry, a loss of approximately $3,600—nearly three times the loss she would experience under the 2010 laws.

Not all couples would lose their EITC benefits were they to marry—many could actually earn a larger EITC within marriage than if they were to remain unmarried. For example, a non-working single mother with two children would receive a $5,036 EITC by marrying a single man earning $14,000. In fact, many women located on the phase-in portion of the benefit structure could receive higher EITC benefits were they to marry their partners than if they remained unmarried. In this way, the EITC creates different incentives for individuals to marry or remain unmarried depending on where they lie on the benefit structure and what their potential spouses earn. Because of this, women of similar earnings levels may be eligible for very different EITC benefits under marriage based on the earnings of their potential spouses.

III. Previous Literature

The traditional economic framework for analyzing marriage behavior began in the 1970s, from work done by Gary Becker. Under the Becker (1974) model, individuals choose to marry if their utility within marriage exceeds their utility outside of marriage. If two individuals are able to combine their resources and improve the total wellbeing of the household, then these two individuals marry. Becker (1981) also suggests that couples who specialize in different markets—one spouse working in the labor market and one specializing in home production—are likely to be better matches. More recent evidence refutes this theory, showing that highly-educated women are more likely to marry (Oppenheimer 1997) and women are increasingly likely to marry partners with similar levels of educational attainment as themselves (Schwartz and Mare 2005). Coinciding with the increase in assortative mating among college-educated individuals in particular is a rise in the prevalence of cohabitation over the last couple of decades, particularly among the less-educated population (Bumpass and Lu 2000; Kennedy and Bumpass 2011; Lichter, Qian, and Mellott 2006; Lundberg and Pollak 2013).

The EITC may have played a role in these trends, particularly among low-income individuals. With the implementation and expansion of the EITC throughout the 1990s and early 2000s, some individuals may gain a large tax credit from marriage, while others are penalized through the same system. If two working individuals can enjoy the same benefits within cohabitation as in marriage, they may choose not to marry if marrying reduces their EITC benefits. This assumes that cohabitation can be viewed as a substitute for marriage—that individuals can enjoy similar benefits within cohabitation as in marriage. This may be true for couples that risk losing social benefits, such as the EITC, if they were to marry, but also depends on differences in how finances are shared within marriage versus cohabitation. The literature on this topic is somewhat mixed, but most studies find some degree of income or expense pooling within cohabiting couples, although generally lower levels of resource pooling than for married couples (DeLeire and Kalil 2005; Kenney 2004; Oropesa, Landale, and Kenkre 2003).[22] While cohabitation has become commonplace in recent years, the vast majority of individuals do wish to marry at some point in the future, when the necessary financial prerequisites have been met (Edin and Kefalas 2005; Smock, Manning, and Porter 2005; Gibson-Davis 2009), suggesting that marriage does serve a different purpose than cohabitation. Still, if couples are able to enjoy similar benefits within cohabitation as in marriage, particularly if they are able to maintain their welfare or EITC benefits, then the costs of losing those benefits may indeed influence whether or

[22] Many of these studies focus on cohabiting couples where both individuals are the biological parents of the children in the household. Due to data limitations, I am unable to distinguish between women cohabiting with the biological father of their children or an unrelated man.

not couples choose to marry.

III.B. EITC and Marriage

Much of the early research on the EITC focused on its impact on labor supply (for a review, see Hotz and Scholz (2003) or Meyer (2010)), with less focus on how the expansion of the EITC has altered union formation decisions. Ellwood (2000) analyzes the effect of the expansion of the EITC and the welfare system on the labor supply of single mothers as well as the impacts on marriage and cohabitation. Using data from the Current Population Survey (CPS), he calculated the potential gain or loss in EITC benefits associated with marriage using a sample of cohabiting couples. He then analyzed the marriage and divorce rates among people who gain, lose, or experience no change in their EITC benefits upon marriage. His findings suggest little evidence of a response in marriage rates to the expansion of the EITC throughout the 1990s, but there is some evidence of an increase in marriage in the late 1990s among individuals who expect to gain EITC benefits from marriage. Ellwood suggested that this might be evidence for future behavioral responses. The EITC is distributed annually through the tax code, and may take multiple years to fully understand by the taxpayer. As taxpayers gain information over time, we might expect behavioral responses to occur several years after a reform has taken place. Now, as 24 states have their own EITCs in addition to a federal EITC reaching over $5,000 in 2012, individuals may have much more to lose (or gain) from marriage, depending on their household income. Further, Ellwood's analysis focused only on cohabiting couples, when potential spouse earnings are known. While the paper does a separate analysis for all single mothers, there is no calculation of potential spouse earnings and expected EITC losses or gains for single mothers who did not cohabit, the vast majority of the sample.

Other recent work has also explored the connection between EITC benefits and family structure (Dickert-Conlin and Houser 2002, Herbst 2011). Herbst (2011) used vital statistics data to analyze how the expansion of the EITC impacted marriage and divorce rates in states with more generous state EITCs over time. He found that increases in the EITC were associated with significant declines in new marriage rates and had virtually no impact on divorce rates. Herbst found that a $1,000 increase in the EITC led to a 4.9% decrease in the new marriage rate in a given state (Herbst, 2011). Despite having detailed information on marriage and divorce rates for all states from 1977-2004, the vital statistics data lack information about personal characteristics such as race, education, and income. Further, Herbst (2011) cannot distinguish between cohabiting couples and individuals not living with partners, which might be an important distinction in determining who is likely to respond to the incentives tied to the credit.

Dickert-Conlin and Houser (2002) used survey data to analyze the impact of the EITC on marriage and divorce rates in the early 1990s using the Survey of Income and Program Participation (SIPP). While these data capture more personal characteristics, many of the reforms made to the EITC have occurred throughout the 1990s and 2000s—increasing the overall value of the benefit, increasing the number of children eligible in calculating the EITC, and increasing the income threshold for married couples. In 2009, the EITC provided additional benefits for up to 3 children and was worth up to $5,666: a -45% tax rate on earnings up to $12,570. In 1993, the last year of the Dickert-Conlin and Houser data, the maximum benefit for two children was $1,511: a -19.5% tax rate on earnings up to $7,750 (Tax Policy Center, 2011). Further, Dickert-Conlin and Houser (2002) did not calculate a potential loss or gain in the EITC upon marriage. Individuals with equal EITC benefits outside of marriage may have vastly different EITC benefits within marriage; level differences in the EITC are not enough to assess the potential costs and benefits associated with marriage.

The studies mentioned thus far have all found minor effects of the EITC on marriage decisions. Many of them were based on somewhat older data, which often do not contain information on cohabiting partners. As the credit has become more generous over time, expected changes in EITC benefits associated with marriage may play an increasingly important role in cohabitation and marriage decisions. Finally, none of the studies mentioned thus far have attempted to account for the types of spouses these individuals marry and the potential losses or gains in EITC benefits upon marriage.

IV. Data

Survey of Income and Program Participation

Data come from a sample of single women between the ages of 18 and 50 from the 2001, 2004, and 2008 Survey of Income and Program Participation, a nationally representative survey of 36,700 households in 2001, 46,500 households in 2004, and 52,000 households in 2008. The data contain detailed information regarding income from various sources for each individual residing in the household. The data are also longitudinal, following individuals for 36 months in 2001, 48 months in 2004, and 60 months in 2008. Its large sample size, coupled with detailed information on earnings and household structure make the SIPP an ideal data source for analyzing marriage and cohabitation behavior in the context of the EITC.

I focused on the sample of unmarried individuals at the beginning of the survey who were eligible for the EITC during the first year of the SIPP survey and identified as the main respondent in the household. I then observed whether these individuals cohabit or marry by the end of the survey window. I restricted the sample to female respondents between 18 and 50 to capture the sample most likely to experience a marriage or cohabitation transition. This yielded a sample of 6,745 individuals. I further restricted the analysis to individuals with a high school degree or less to best approximate the population of EITC recipients, 3,058 individuals. While eliminating individuals with at least some college experience reduced the sample by more than half, EITC recipients with college experience are likely to be more transient in their participation, as they typically have earnings well above the eligibility threshold for the EITC. [23]

Using information about family earnings in the first calendar year of the SIPP survey, I calculated the federal and state EITC the respondent expected to receive given the number of qualifying children residing in the household in the first month of the survey. A qualifying child is a biological child, adopted child, sibling, or descendent of any of these (such as grandchild or niece/nephew) who resides in the home for at least 6 months, or a foster child who lives in the house for the entire year. Further, all qualifying children must be under 19 years old, 24 years old and a full-time student, or any age and permanently disabled (Internal Revenue Service 2013).

The SIPP provides limited information on demographic characteristics; I control for age, race, number of children living in the household, whether the individual was previously married, whether the individual had any private health insurance during the first year of the SIPP survey, and whether their potential spouse had volatile income. Number of children living in the household, marriage histories, health insurance status, and spouse income volatility are all included as potential indicators of marriage desirability. Women with children living in the household may be less desirable marriage partners because they come with extra responsibility and potential complications with previous partners. Women who have never been married before may have unobservable characteristics that make them less likely to marry at all. Women who lack private health insurance may be more likely to marry in order to gain health insurance coverage from a spouse. Finally, there has been increasing evidence that women look for financially-stable men when making marriage decisions (Edin and Kefalas 2005; Gibson-Davis

[23] As a falsification check, I also ran the analyses on individuals with some college or more to illustrate how higher-educated women are not likely to make marital or cohabitation decisions based on the EITC due to their transient eligibility.

2005), so I control for whether the predicted spouse had 'volatile' income, as defined by a decline in income by more than 50% from year 1 to year 2 of the SIPP survey.

V. Empirical Strategy

V.A. Predicting Potential Spouse Income

Estimating the potential losses or gains in EITC benefits associated with marriage requires first estimating the earnings of the potential spouse. For the majority of the sample, spouse earnings are unobservable because individuals do not cohabit or marry during the survey window. For individuals who do cohabit or marry, there may be concern that couples adjust their labor force participation in response to the union formation, and thus any calculation of a loss or gain in EITC benefits will reflect these post-union formation labor force responses. To address both of these issues, I simulate a marriage market for all individuals in the sample, regardless of whether a partner is observed at any point in the survey. This reduces concerns of measuring spouse earnings after the marriage or cohabitation decision, and also allows for calculation of the loss or gain in EITC benefits upon marriage for individuals who are never observed living with a partner during the survey window.

I employ a strategy similar to that of Bertrand, Kamenica, and Pan (2013), where separate marriage markets are constructed based on the race, age and education of the individuals in the sample and couples are randomly matched based on these demographic characteristics. Rather than placing individuals in a specific marriage market based on these characteristics, I first observe the characteristics of couples that are already married in the CPS March Supplement and use these matches to construct probabilities that specific matches will occur among single individuals. Pooling data from the 2002-2011 CPS March supplements, there are 90,900 married women. I divided these women into four race categories—white, black, Hispanic, and Asian/all others; four education categories—less than high school degree, high school degree, some college, and college degree or more; and six age categories—19–24, 25–29, 30–34, 35–39, 40–49, and 50+. I then divided their spouses into the same categories and created a 96x96 matrix, resulting in 9,216 different age-race-education spouse match possibilities. I then calculated the percent of married women in each age-race-education cell married to a man in each age-race-education cell. A table summarizing these findings can be found in Appendix Table 1. As one might expect, there is considerable educational and racial homogamy—for example, roughly 90% of women are married to men of the same race. Similarly, women with less than a high school degree are more likely to marry men with less than a high school degree than men with a college degree. Finally, women in each age cell are much more likely to marry men of their age, or one age category older.

After calculating the probabilities of a woman of each type marrying each potential partner type, I then simulate a marriage market with the sample of women from the SIPP and a sample of single men aged 18-62 in the 2001, 2004, and 2008 SIPP panels, a sample of 30,718 men. For each single woman in the SIPP, I randomly drew 30,718 potential spouses with replacement from the SIPP and created a 'successful' match once finding the first man with the highest probability of matching, according to the probabilities generated from the sample of marriages in the CPS. I then calculated a potential EITC benefit under marriage for each match and quantified the expected loss or gain in EITC benefits upon marriage.

A distribution of the expected change in EITC benefits upon marriage is shown in Figure 2. Figure 2 shows the difference between the household EITC benefit if the women remained single and the household EITC benefit if the women married their potential spouses. Positive numbers indicate that the EITC benefit remaining single is higher than the benefit under marriage. The average change in EITC benefits upon marriage is a $1,000 (2011$) loss in benefits, with some individuals in the sample losing over $6,000 in EITC benefits upon marriage. On the other hand, a few individuals in the sample actually *gain* $6,000 in EITC benefits upon marriage. It is clear from this figure though, that the majority of women would receive a lower EITC benefit upon marriage. In fact, approximately 65 percent of the sample would lose some of their EITC benefits upon marriage.

V.B. Validation of Spouse Match

Identifying whether individuals can expect to gain or lose EITC benefits upon marriage relies heavily on the quality of the spouse match. The average woman in the sample is matched to a potential spouse that earns approximately $24,000—$6,000 higher than the $18,000 observed for women who either cohabit or marry by the end of the SIPP survey. To check the quality of the matches made in the simulated marriage market, I compare the earnings distributions of the predicted spouses to the actual spouses or partners for the women in the sample who actually cohabit or marry. Appendix Figure 1 illustrates the distribution of predicted and observed spouse earnings for individuals who either cohabit or marry by the end of the SIPP survey. 95% confidence intervals are illustrated with black bars.

Appendix figure 1 illustrates that the predicted spouse earnings are no different from the observed spouse earnings for the lower half of the distribution of spouse earnings. For individuals in the 60th percentile or higher, the predicted partner earnings are significantly higher than the actual spouse or partner earnings observed. This pattern is not entirely unexpected. The simulated marriage market only uses age, race, and education to evaluate the quality of matches and cannot take into account other characteristics of the individuals such as prior childbearing history or unobserved characteristics that might make individuals more or less desirable as marriage partners. As the sample of EITC-eligible women is predominantly low-income single mothers, there are likely unobserved characteristics that prevent them from marrying higher-earning spouses, but I am unable to observe these characteristics when matching them to potential spouses. Thus the simulated marriage market is likely to overestimate the quality of men that these single women marry.

Since the differences in the earnings distributions are evident only in the top half of the earnings distribution, this is unlikely to substantially affect the calculation of the expected loss in EITC benefits upon marriage. The earnings of men in the top half of the earnings distribution are likely to render all of these women ineligible for the EITC. Despite this, I take a number of steps to attempt to account for these differences in the earnings distribution of potential spouses compared to the observed spouses. I first top-code the earnings of single men in the marriage market to $75,000, since none of the women in this sample go on to marry men who earn more than $75,000. This reduces the difference in the earnings between the predicted and observed spouses for individuals at the very top of the earnings distribution. Second, as a robustness check, I conduct separate analyses on the sub-group of women who are matched to men in the bottom half of the earnings distribution, since there are no significant differences between the predicted and observed earnings of spouses for this group. It is likely that the women marrying men in the bottom half of the earnings distribution are the ones most likely to alter their marriage or cohabitation behavior based on the value of their EITCs.

V.C. Multinomial Logistic Regression

After assessing the change in EITC benefits upon marriage, I next run a multinomial logistic regression to estimate the likelihood of transitioning from single to cohabitation or marriage by the end of the SIPP survey.[24] The conditional probability of marrying or cohabiting is modeled as:

[24] A few individuals experience a transition to both cohabitation and marriage over the time period, so I code these individuals as married by the end of the survey. Approximately 4% of the sample experiences both a cohabitation and a marriage within the 36-48 month surveys.

$$P(K=1|x) = \frac{e^{\beta_{1k}EITC+\beta_{2k}\Delta EITC+\beta_{3k}SPEARN+\delta_k x+\alpha_t+\varepsilon}}{\sum_{j=1}^{J} e^{\beta_{1j}EITC+\beta_{2j}\Delta EITC+\beta_{3j}SPEARN+\delta_j x+\alpha_t+\varepsilon}}$$

Where k represents the outcomes of interest, cohabiting or married, $\boldsymbol{\beta}_{ik}$ are the outcome-specific coefficients on the EITC variables of interest—the dollar value of the EITC before marriage, the amount of expected change in EITC benefits, and a quadratic form of predicted spouse earnings. **X** is a vector of personal characteristics including education, race, age, and number of children. $\boldsymbol{\alpha}_t$ is a set of year-specific controls, which allows differences across SIPP surveys, $\boldsymbol{\varepsilon}$ is an individual error term. I assume there are **j** states over which the individual can choose—single, cohabiting, or married.

As there are now 24 states that supplement the federal EITC, I also control for differences across states with their own EITCs in two different ways. In one model, I include an indicator for whether the state of residence has its own EITC, and in another, I include state fixed effects. State EITC benefits are also included in the calculation of the total benefit amount and the expected change in benefits upon marriage.

Variation in the value of the expected loss in EITC benefits come from two primary sources. The first is through the spouse match, where respondents are randomly matched to single men based on the race, age, and education of both the respondent and the spouse. Because I also control for demographic characteristics in the analyses, the effect of the expected loss in EITC benefits is not due to differences in marriage or cohabitation patterns by race or education, but due to the random variation in earnings generated by the spouse match.

Second, as mentioned in the background section, there have been federal and state policy changes to the EITC benefit structure over the observed time period. Starting in 2002, the EITC benefit structure was expanded to allow married couples to earn more than single individuals and maintain the same EITC benefit. Because I calculate the expected EITC loss based on the first year of earnings observed in each of the three SIPP panels, some of the variation in these losses will be generated from the different policies. In 2001, there was no extra allowance for married couples, in 2004 there was a $1,000 allowance for married couples before benefits were phased out, and in 2009 married couples could earn $5,000 more than single filers before their benefits were phased out. Because these changes were implemented at the federal level, it is difficult to distinguish any effect of these policy changes from other general time trends that may have occurred between 2001 and 2009. In general, marriage rates have declined in recent years, which would counteract any positive effect of these policy changes on marriage rates, but individuals were also followed for a longer duration in the 2008 SIPP than the 2001 SIPP, providing a longer window of opportunity for a marriage to occur.[25] In addition to the federal policy changes, several states have implemented their own supplemental EITCs over this time period, providing an additional source of variation in expected EITC losses. Because state EITCs are based on the federal EITC, if individuals expect to experience a reduction in their federal EITC upon marriage, they will also experience a reduction in their state EITC benefit.

V.D. Summary Statistics

Summary statistics are shown in Table 1, illustrating differences in characteristics between those who expect to lose benefits, gain benefits, or experience no change in benefits upon marriage. Among those who would lose EITC benefits upon marriage, the average loss in benefits was $2,130, or about 80% of the EITC benefits they would receive if they remained single. Those who would gain EITC benefits upon marriage tended to have lower earnings than

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[25] Individuals were followed for 36 months for the 2001 SIPP, 48 months for the 2004 SIPP, and 60 months for the 2008 SIPP. Nearly 80% of all marriages observed in this sample from the 2004 and 2008 SIPP occurred within 36 months of the start of the panel.

those who would lose benefits ($3,500 in earnings compared to $14,000), which would likely place them on the phase-in portion of the EITC benefit schedule. These women would experience an approximate 70% increase in their EITC benefits were they to marry, increasing their EITC benefits from $1,903 to $3,309. Consistent with predictions that losses in EITC benefits upon marriage should deter marriage, marriage rates are lower among those who expect to lose EITC benefits upon marriage compared to those who would gain benefits (12% compared to 17%), and cohabitation rates are approximately equal across groups (11% of the sample).

Demographic differences are also apparent between individuals expecting to lose benefits and those expecting to gain benefits upon marriage. In general, these differences would predict higher marriage rates among individuals who expect to lose EITC benefits as these individuals are more likely to be white, have a high school diploma, and have slightly fewer children—all characteristics that have been shown to be positively associated with marriage.

Table 2 shows patterns of EITC eligibility across the three SIPP panels used in this analysis. Policy changes to reduce the marriage penalty in the EITC between 2001 and 2008 were associated with a reduction in the likelihood that a single woman would experience a loss in EITC benefits upon marriage (71% in the 2001 SIPP compared to 53% in the 2008 SIPP), and a smaller loss for individuals who did experience a reduction in benefits ($1,800 in 2001 compared to $1,200 in 2008 SIPP (2011$)), despite similar levels of pre-marriage EITC benefits ($2,500 in both surveys (2011$)). Coinciding with this reduction in the marriage penalty associated with the EITC, marriage rates were also higher in the 2008 SIPP than in the 2001 SIPP, with 17% of the sample marrying in 2008 compared to 12% marrying in 2001.

VI. Results

Table 3 presents results from the multinomial logistic regressions predicting decisions to remain single, cohabit, or marry as a function of EITC benefits. The multinomial logistic regression jointly models the probability of cohabiting and marrying, in comparison to remaining single throughout the survey. All models use the sample of women who remain single throughout the survey as the reference group; standard errors are clustered at the state level. All values reported are average marginal effects. For indicator variables, the coefficient represents the discrete change in the outcome when the indicator variable increases from 0 to 1. The first model uses only a linear form of the expected change in EITC benefits upon marriage (measured in thousands of dollars), based on the predicted spouse's earnings. With no other controls, a $1,000 increase in expected EITC loss is associated with virtually no change in the likelihood of cohabiting, and a 0.7 of a percentage point decline in the likelihood of marrying over the course of the SIPP survey.

Model 2 includes controls for demographic characteristics and whether the individual lives in a state with its own EITC. Once adding these controls, the effect of a $1,000 increase in EITC losses is associated with a slight, 0.3 of a percentage point increase in the likelihood of cohabiting, and a 0.9 of a percentage point decline in the likelihood of marrying. Controlling only for the change in EITC benefits upon marriage masks the large increase in household earnings associated with the expected loss in EITC benefits. In Model 3, I add a quadratic control for expected spouse earnings and an indicator for whether the spouse has volatile income—defined as a drop in income by more than 50% over two years. After controlling for spouse earnings and the total amount of EITC benefits received while single, a $1,000 loss in EITC benefits is associated with a 0.9 of a percentage point increase in the probability of cohabiting, and a 1.6 percentage point decline in the probability of marrying. Since about 11% of the sample experiences a cohabitation and 13% experience a marriage over the course of the survey, these results represent a 8-12% change in marriage and cohabitation rates associated with a $1,000 loss in EITC benefits.

Spouse earnings have a slight positive association with marriage—a $1,000 increase in spouse earnings increases one's likelihood of marrying by 0.1 of a percentage point. Not surprisingly, this suggests that women not only respond to expected changes in their EITC benefits upon marriage, but also to the overall increase in household income. Women seem to have different criteria about the men they choose to cohabit with than the men they choose to marry: spouse earnings significantly increase the likelihood that a woman marries, but *decrease* the likelihood that she cohabits. This supports findings in the qualitative literature that low-income women look for men with stable jobs and financial stability when choosing marriage partners (Edin and Kefalas 2005) and that high earnings are not a necessary condition to cohabit with a partner (Edin 2000; Smock, Manning, and Porter 2005). In order to preserve traditional gender identities, men and women may also be more reluctant to marry in situations where the woman out-earns her husband (Bertrand et al. 2013).[26] I find no effect of volatile income on the likelihood of cohabiting or marrying, although the coefficients point in the expected direction—individuals matched to spouses with volatile income are slightly less likely to marry than those matched to spouses with more stable income. Finally, in Model 4, rather than including an indicator variable for whether the individual lives in a state with an EITC, I include a set of state fixed effects. The results are qualitatively very similar, with expected changes in EITC benefits yielding a 1.1 percentage point increase in the likelihood of cohabiting, and a 1.8 percentage point decline in the likelihood of marrying by the end of the SIPP survey.

Demographic controls perform as expected—black women are significantly less likely to marry and cohabit than white women, and Hispanic women are more likely to marry and less likely to cohabit than white women. Women without a high school degree are significantly more

[26] In results not shown, I also found that modeling spouse earnings using an indicator for whether the respondent out-earned her potential spouse had a negative association with the likelihood of marrying.

likely to cohabit and significantly less likely to marry compared to women with a high school degree. Surprisingly, the number of children in the household does not seem to have an effect on either cohabitation or marriage. I expected that children might deter women from marrying, or make them less attractive marriage partners than childless women. However, as most EITC-recipients have children (85%), perhaps the actual number of children living in the household does not significantly alter one's likelihood of marrying or cohabiting.[27] As expected, women who have never been married before are 5 percentage points less likely to marry and 6 percentage points more likely to cohabit, compared to women who have been married before. Finally, I find evidence consistent with Herbst (2011) that states with EITCs have lower marriage rates. Women living in states with their own EITCs are 4 percentage points less likely to marry and 2 percentage points more likely to cohabit. Finally, individuals are more likely to marry in the 2008 SIPP panel than in the 2001 SIPP, which may either reflect the decline in the marriage penalty associated with the EITC, or the longer duration over which individuals are observed in the 2008 SIPP compared to the 2001 SIPP.

To put these result in the context of marriage and cohabitation rates, I next turn to a simulation exercise using results generated from Model 4 of the multinomial logistic regression in Table 3. Under the baseline model (expected EITC loss is about $1,060), about 11% of the sample cohabits during the survey, and about 14% marry. If no one experienced a loss in EITC benefits upon marriage, results from the multinomial logistic regression predict that the cohabitation rate would drop to 10% and the marriage rate would increase to 16%. This exercise suggests that the introduction of the marriage penalty in the EITC increased the cohabitation rate by about 1 percentage point on a base of 11 percent (a 9% increase), and decreased the marriage rate by 2 percentage points on a base of 14 percent (a 14% decrease).

[27] Alternate measures of children in the household were also not significant such as whether the respondent had any children at all, or whether the respondent had two or more children.

VI.B. Falsification and Robustness Checks

I next conduct an analogous analysis for women with some college experience or a college degree. Table 4 shows these results. The first two columns of Table 4 replicate the results from Model 3 in Table 3 for women with a high school degree or less who are eligible for the EITC. The next two columns show the same model for a sample of women with at least some college experience. All of these women were eligible for the EITC at the beginning of the SIPP survey, but the higher educated women are likely to have higher earnings in subsequent years. Indeed, I find virtually no effect of the EITC and income on marriage and cohabitation decisions for women who have at least some college experience. Interestingly enough, while the earnings of potential spouses is positively associated with marriage for the low-educated sample, I find no statistically significant relationship between spouses' earnings and the likelihood of marriage for the highly-educated sample. This is consistent with some work suggesting that low-educated couples may be particularly averse to situations where the wife out-earns her husband (Bertrand et al. 2013). Further, the highly-educated sample is also less affected by whether the state has an EITC benefit of its own. While states with EITC benefits experienced a 4 percentage point decline in the marriage rate among women with a high school degree or less, there is no clear association between state EITCs and marriage and cohabitation patterns among the highly-educated sample.

As discussed throughout, the results of these analyses rely heavily on the assumptions made regarding potential spouse matches. Appendix figure 1 illustrated that the predicted earnings of spouses generated by the marriage market were generally higher than the actual earnings of spouses or partners observed by the sub-sample of women who marry or cohabit within the SIPP panel. This pattern is more pronounced among the top half of the spouse earnings distribution, and also relies on the information from the relatively few respondents in this sample who experience a marriage or cohabitation during the SIPP panel. As a robustness check, I conducted the same analyses presented in Table 3 for the subsample of women whose potential spouses' earnings fall within the bottom half of the earnings distribution, where there are no significant differences between the actual and predicted spouse earnings. Results from this exercise are presented in Table 5. In each of the models, the magnitude of the effect of the expected loss in EITC benefits is similar or larger for the sub-sample of individuals in the bottom half of the spouses' earnings distribution than for the sample as a whole. This suggests that the results presented in Table 3 are not driven solely by individuals matched to high-earning spouses. Since the maximum household earnings threshold for EITC eligibility is around $45,000, relatively small changes in household income could have substantial consequences for EITC eligibility—spouses earning more than $25,000 are likely to render most EITC-eligible women in this sample ineligible for the EITC entirely. Relatively small differences between the actual and predicted earnings of spouses are unlikely to have an impact on the magnitude of the expected EITC loss upon marriage, particularly for spouses in the top half of the earnings distribution.

VII. Conclusion

The EITC has been a widely popular program due to its success in lifting millions of households out of poverty. It has been shown to dramatically increase the labor supply of single mothers (Ellwood 2000, Meyer and Rosenbaum 2000) and evidence suggests positive outcomes for child wellbeing (Dahl and Lochner 2012) and maternal health (Evans and Garthwaite 2010). Previous evidence on the effect of the EITC on marriage and divorce suggests small, negative impacts on marriage and virtually no impact on divorce (Dickert-Conlin and Houser 2000, Ellwood 2000, Herbst 2011). Previous studies, with the exception of a small analysis in Ellwood (2000) on cohabiting couples, have not attempted to account for potential spouse earnings in calculating the incentives or disincentives to marry. Further, none of these studies have considered the effects of the EITC on cohabitation rates.

Using a sample of single men from the SIPP, I predicted spouse earnings for a sample of single women in the SIPP who were eligible for the EITC. Results show that most women eligible for the EITC can expect to lose some of their EITC benefits were they to marry. I find an expected loss of about $2,100 for women who expect to lose benefits upon marriage, an 80% decline in their pre-marriage EITC benefits. I find that a $1,000 expected loss in EITC benefits upon marriage increases the likelihood of cohabiting by 1.1 percentage points and decreases the likelihood of marrying by 1.8 percentage points. Simulating changes in cohabitation and marriage rates using results from multinomial logistic regressions suggest that were there no marriage penalty in the EITC, the cohabitation rate among this sample would decline from 11 percent to 10 percent and the marriage rate would increase from 14 percent to 16 percent.

The results of this analysis rely heavily on the calculation of the expected EITC losses associated with marriage. As many of the women in this sample do not marry over the course of the SIPP survey, spouse earnings are unobservable for the majority of the sample. To address this problem, I rely on data from the CPS sample of married women to predict the types of men women marry. There are inherently some assumptions made when predicting these potential spouses, namely that the sample of married women have similar marriage prospects as the single women. This is unlikely to be true, particularly as many of the single women eligible for the EITC have children, which may alter their marriage prospects significantly (Graefe and Lichter 2007). Without having information on whether the married women in the CPS had children prior to the marriage, it is difficult to assess the impact of children on marriage matches. In addition, the CPS lacks information on the duration of the marriage, so a cross-section of existing marriages emphasizes more stable marriages. Despite these assumptions, the predicted spouse earnings distribution looks quite similar to the actual spouse earnings distribution for individuals for whom spouse earnings are observed in the SIPP, particularly for individuals in the bottom half of the earnings distribution. Results were consistent when excluding individuals predicted to marry spouses in the top half of the earnings distribution, where the predicted spouse earnings differed the most from the observed spouse earnings.

Beyond the impact of expected losses in EITC benefits, there are other findings that also indicate a relationship between marriage and EITC benefits. Women living in states that supplemented the federal EITC were nearly 4 percentage points less likely to marry than women living in states without EITCs, suggesting that larger EITC benefits (and thus larger potential losses in EITC benefits) may be discouraging women from marrying. Expected spouse earnings play a role in marriage and cohabitation decisions as well. A $1,000 increase in spouse earnings is associated with a 0.1 of a percentage point increase in the probability of marriage and a 0.2 of a percentage point decline in the probability of cohabiting by the end of the SIPP. This result fits in with literature analyzing the marriage patterns of low-income women, which suggests that women look for financial stability when looking for marriage partners (Blau, Kahn, and Waldfogel 2000; Edin and Kefalas 2005) but that the criteria for cohabitation is different from that of marriage (Smock, Manning, and Porter 2005).

When conducting the same analysis on a sample of EITC-eligible women with some college or more educational attainment, I find no effect of EITC benefits on marriage and cohabitation decisions. This suggests that individuals who are likely more transient recipients of the EITC are less responsive to the marriage disincentives in the benefit structure, perhaps because they do not expect to receive the EITC for consecutive years.[28] I also found no relationship between potential spouse earnings and the likelihood of marriage for the highly-educated sample, which supports other findings that low-educated individuals may be more sensitive to intra-household divisions of earnings (Bertrand et al. 2013).

These results suggest that low-income single women with a high school degree or less are more likely to cohabit and less likely to marry their partners if they expect to lose EITC benefits upon marriage. If individuals could receive the same EITC benefit while married as they did when they were single, results from this analysis suggest that the cohabitation rate in this sample would drop by about 9%, and the marriage rate would increase by 14%. This suggests that EITC recipients do respond to financial incentives to marry or cohabit with their partners and the benefit structure of the EITC may be influencing these decisions. Further, as approximately 65 percent of this sample of unmarried women would lose EITC benefits upon marriage, eliminating the marriage disincentives in the EITC benefit structure would likely affect the marriage and cohabitation decisions of millions of low-income families.

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Historical EITC parameters. Viewed on 4/30/11

Table 1. Descriptive statistics by expected loss or gain of EITC benefits upon marriage, 18-50 year old EITC-eligible women with a high school degree or less

	Smaller EITC upon marriage	No Change in EITC upon marriage	Larger EITC upon marriage	All
Married by the end of the survey	0.12	0.14	0.17	0.135
Cohabiting by the end of the survey	0.11	0.12	0.11	0.115
Race/Ethnicity				
White	0.45	0.38	0.42	0.43
Black	0.29	0.32	0.31	0.30
Hispanic	0.23	0.26	0.23	0.23
Asian/Native American	0.03	0.03	0.04	0.04
Education				
Less than a HS diploma	0.29	0.36	0.35	0.31
High school graduate	0.71	0.64	0.65	0.69
Household Characteristics				
Proportion with no private health insurance	0.45	0.57	0.67	0.51
Proportion never married	0.51	0.59	0.58	0.54
Has children	0.82	0.88	0.88	0.84
Number of children in household	1.52	1.59	1.73	1.58
Age	35.1	33.2	33.5	34.5
EITC (2011$)				
Proportion living in a state with EITC	0.32	0.33	0.32	0.32
EITC under marriage	580	2,669	3,309	1,462
EITC single	2,710	2,669	1,903	2,532
Expected difference between EITC while single and EITC married (EITC(single)-EITC(married))	2,130	-	-1,406	1,070
Actual difference in EITC benefits upon marriage (for those who marry or cohabit)	2020		-597	190
Earnings (2011$)				
Individual earnings	13,884	12,516	3,516	12,398
Predicted spouse earnings	34,123	471	9,627	24,075
Actual spouse earnings (for those who marry or cohabit)	19,570	15,889	15,336	18,063
Spouse's income is volatile	0.26	0.02	0.19	0.21
Respondent out-earns potential spouse	0.25	1.00	0.45	0.40

Number of Observations	1,949	425	684	3,058

Source: 2001, 2004, and 2008 Survey of Income and Program Participation. Women with a high school degree or less eligible for the EITC in the first year of the survey, aged 18-50. All dollars are 2011$

Table 2. Trends in EITC eligibility by SIPP survey, 18-50 year old women with less than college and positive EITC benefit in year 1 of survey

	2001 SIPP	2004 SIPP	2008 SIPP
EITC trends in year 1 of SIPP panel			
EITC benefit (unmarried 2011$)	2,513	2,579	2,512
EITC benefit (married 2011$)	1,154	1,375	1,956
Expect to lose EITC benefits upon marriage	0.72	0.66	0.53
Expected EITC loss (year 1 2011$)	1,360	1,204	551
Earnings in year 1 of SIPP panel			
Respondent earnings (2011$)	11,270	11,917	11,962
Respondent + potential spouse earnings (2011$)	33,649	34,022	32,585
Trends over the course of the SIPP panel			
Ever lose EITC benefits over course of panel	0.82	0.79	0.67
Number of years where loss is expected (out of 3)	1.75	1.64	1.28
Total expected loss in EITC benefits over 3 years (2011$)	3,282	3,006	1,626
Average annual expected loss in EITC benefits (2011$)	1,132	1,087	526
Union formation patterns			
Cohabited by end of survey	0.10	0.14	0.11
Married by end of survey	0.12	0.12	0.17
Number of Observations	988	1142	929

Source: 2001, 2004, and 2008 Survey of Income and Program Participation. Women with a high school degree or less eligible for the EITC in the first year of the survey, aged 18-50. All dollars are 2011$

Table 3. Results from Multinomial Logistic Regressions: Unmarried 18-50 year old EITC-eligible women with a high school degree or less

	Model 1		Model 2		Model 3		Model 4	
	Cohabit	Marry	Cohabit	Marry	Cohabit	Marry	Cohabit	Marry
	vs. Remain Single		vs. Remain Single		vs. Remain Single		vs. Remain Single	
EITC								
Expected EITC loss upon marriage	0.002	-0.007 **	0.003 *	-0.009 ***	0.009 ***	-0.016 ***	0.011 **	-0.018 ***
	(.001)	(.003)	(.002)	(.002)	(.003)	(.003)	(.005)	(.005)
EITC value (in thousands)			-0.003	0.007 ***	-0.008 **	0.013 ***	-0.009 *	0.016
			(.003)	(.002)	(.004)	(.003)	(.005)	(.006)
Live in a state with an EITC			0.023 **	-0.039 **	0.023 ***	-0.040 **		
			(.0091)	(.0156)	(.009)	(.016)		
Potential spouse earnings (in thousands)					-0.002 ***	0.001 **	-0.002 **	0.001
					(.001)	(.001)	(.001)	(.001)
Spouse Earnings 'volatile'					0.011	-0.009	0.013	-0.012
					(.009)	(.01)	(.0135)	(.015)
Demographics								
Age			0.009 ***	0.004	0.009 ***	0.004	0.009	0.005
			(.0032)	(.007)	(.003)	(.007)	(.006)	(.007)
Education (Ref= HS grad)								
Less than high school degree			0.025 **	-0.032 ***	0.025 **	-0.032 ***	0.029 **	-0.038 ***
			(.0104)	(.007)	(.01)	(.007)	(.013)	(.015)
Race (ref = White)								
Black			-0.104 ***	-0.065 ***	-0.104 ***	-0.065 ***	-0.136 ***	-0.065 ***
			(.0099)	(.0102)	(.01)	(.01)	(.017)	(.017)
Hispanic			-0.018 **	0.023 ***	-0.018 **	0.024 ***	-0.027 *	0.032 **
			(.0077)	(.0059)	(.008)	(.006)	(.015)	(.016)
Household characteristics								

Number of children in household			-0.004		-0.001		-0.002		-0.005		-0.001		-0.007	
			(.0058)		(.0041)		(.006)		(.004)		(.008)		(.008)	
Respondent has never been married			0.060	***	-0.056	***	0.059	***	-0.057	***	0.065	***	-0.058	
			(.0064)		(.013)		(.006)		(.013)		(.013)		(.014)	
Respondent has no private health insurance			-0.031	**	-0.013		-0.029	**	-0.017		-0.034	***	-0.011	***
			(.0132)		(.0175)		(.013)		(.017)		(.012)		(.013)	
SIPP Panel (ref=2001)														
2004			0.026	**	-0.010		0.027	**	-0.011		0.031	**	-0.016	
			(.013)		(.008)		(.013)		(.008)		(.014)		(.015)	
2008			0.014		0.041	***	0.018		0.036	**	0.020		0.030	**
			(.011)		(.014)		(.011)		(.015)		(.015)		(.015)	
State Fixed Effects											X		X	
Observations in each cell	349	421	349		421		349		421		349		421	
Total number of observations		3,058			3,058				3,058				3,058	

Source: 2001, 2004, and 2008 Survey of Income and Program Participation. Women with a high school degree or less eligible for the EITC in the first year of the survey, aged 18-50. All dollars are 2011$

Note: Analytical marginal effects shown, indicator variables evaluated as a change from 0 to 1. Standard errors clustered at state level. *** indicates significant difference at p<.01, ** p<.05, * p<.10

Table 4. Results from multinomial logistic regressions: Unmarried 18-50 year old EITC-eligible women. By level of educational attainment

	High school degree or less				Some college or more			
	Cohabit		Marry		Cohabit		Marry	
	vs. Remain Single				vs. Remain Single			
Expected EITC loss upon marriage	0.009	***	-0.016	***	-0.002		0.003	
	(.003)		(.003)		(.002)		(.003)	
EITC value (in thousands)	-0.008	**	0.013	***	0.004		-0.005	
	(.004)		(.003)		(.002)		(.004)	
Potential spouse earnings (in thousands)	-0.002	***	0.001	**	0.000		-0.001	
	(.001)		(.001)		(.)		(.001)	
Spouse earnings 'volatile'	0.011		-0.009		0.005		-0.015	
	(.009)		(.009)		(.008)		(.009)	
Live in a state with an EITC	0.023	***	-0.040	**	-0.006		-0.009	
	(.009)		(.016)		(.006)		(.007)	
Demographics								
Age	0.009	***	0.004		0.004		0.012	***
	(.003)		(.007)		(.003)		(.003)	
Education (Ref= HS grad)								
Less than high school degree	0.025	**	-0.032	***	n/a		n/a	
	(.01)		(.007)					
Race (ref = White)								
Black	-0.104	***	-0.065	***	-0.056	***	-0.083	***
	(.01)		(.01)		(.005)		(.009)	
Hispanic	-0.018	**	0.024	***	0.007		-0.019	**
	(.008)		(.006)		(.015)		(.007)	
Household characteristics								
Number of children in household	-0.002		-0.005		0.003		0.021	***
	(.006)		(.004)		(.004)		(.004)	
Respondent has never been married	0.059	***	-0.057	***	0.047	***	-0.048	***
	(.006)		(.013)		(.008)		(.013)	
Respondent has no private health insurance	-0.029	**	-0.017		0.005		-0.040	***
	(.013)		(.017)		(.006)		(.007)	
SIPP Year Fixed Effects	X		X		X		X	
Observations in each cell	349		421		856		1303	
Number of observations	3058				9628			

Source: 2001, 2004, and 2008 Survey of Income and Program Participation. Women eligible for the EITC in the first year of the survey, aged 18-50.

Note: Analytical marginal effects shown, indicator variables evaluated as a change from 0 to 1. Standard errors clustered at state level. All values in 2011$. *** indicates significant difference at p<.01, ** p<.05, * p<.10

Table 5. Results from Multinomial Logistic Regressions: Unmarried 18-50 year old EITC-eligible women with a high school degree or less; individuals matched to spouses in bottom half of income distribution

	Model 1				Model 2				Model 3			
	Cohabit	Marry			Cohabit		Marry		Cohabit		Marry	
	vs. Remain Single				vs. Remain Single				vs. Remain Single			
EITC												
Expected EITC loss upon marriage (bottom half of earnings sample)	0.004	-0.013	*		0.007	*	-0.015	**	0.008	**	-0.015	**
	(.003)	(.007)			(.004)		(.007)		(.004)		(.007)	
Demographic controls					X		X		X		X	
Spouse earnings									X		X	
SIPP Year Fixed Effects					X		X		X		X	
Observations in each cell	199	225			199		225		199		225	
Total number of observations		1,680					1,680				1,680	

Source: 2001, 2004, and 2008 Survey of Income and Program Participation. Women with a high school degree or less eligible for the EITC in the first year of the survey, aged 18-50. All dollars are 2011$

Note: Analytical marginal effects shown, indicator variables evaluated as a change from 0 to 1. Standard errors clustered at state level. All values in 2011$. *** indicates significant difference at p<.01, ** p<.05, * p<.10

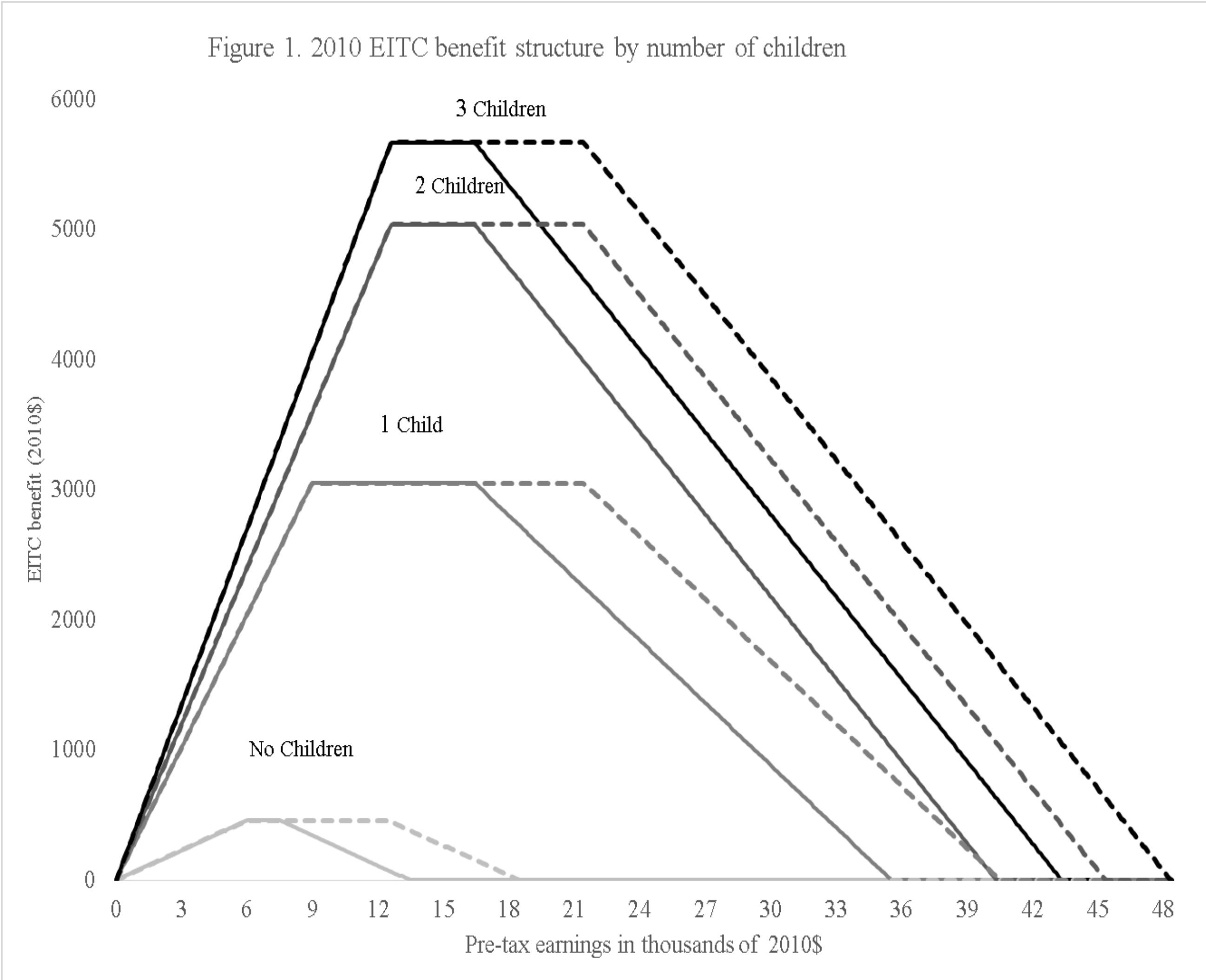
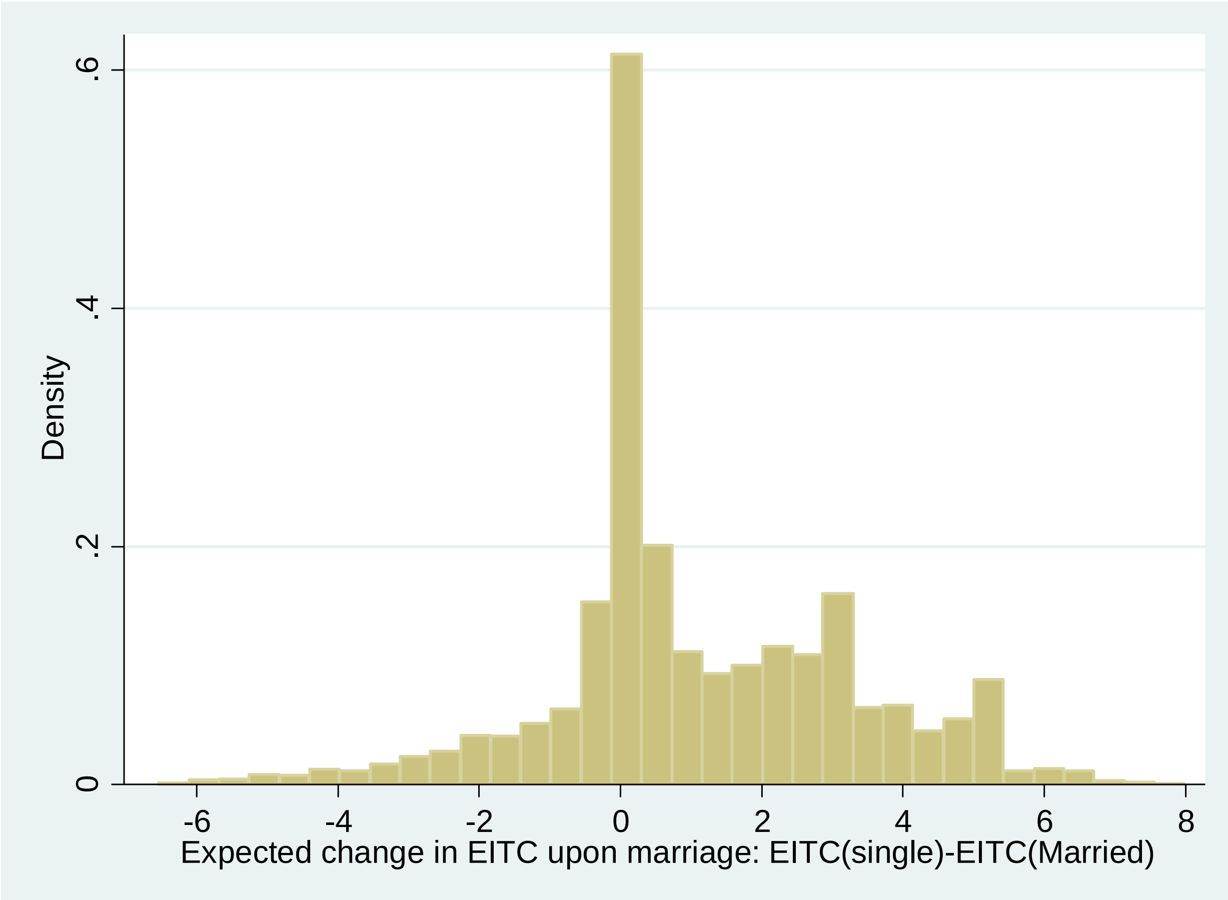


Figure 1. 2010 EITC benefit structure by number of children
3 Children
2 Children
1 Child
No Children
EITC benefit (2010$)
6000
5000
4000
3000
2000
1000
0
0
3
6
9
12
15
18
21
24
27
30
33
36
39
42
45
48
Pre-tax earnings in thousands of 2010$

Figure 2. Distribution of expected EITC loss (in thousands of dollars) among EITC-eligible women



Source: 2001, 2004, and 2008 Survey of Income and Program Participation. Sample of women aged 18-50 who are eligible for the earned income tax credit and have a high school degree or less. Note: Positive values correspond to higher benefits while single than while married (an expected loss in EITC benefits upon marriage). Negative values correspond to higher EITC benefits upon marriage. Expected changes in benefits measured in thousands of 2011$.

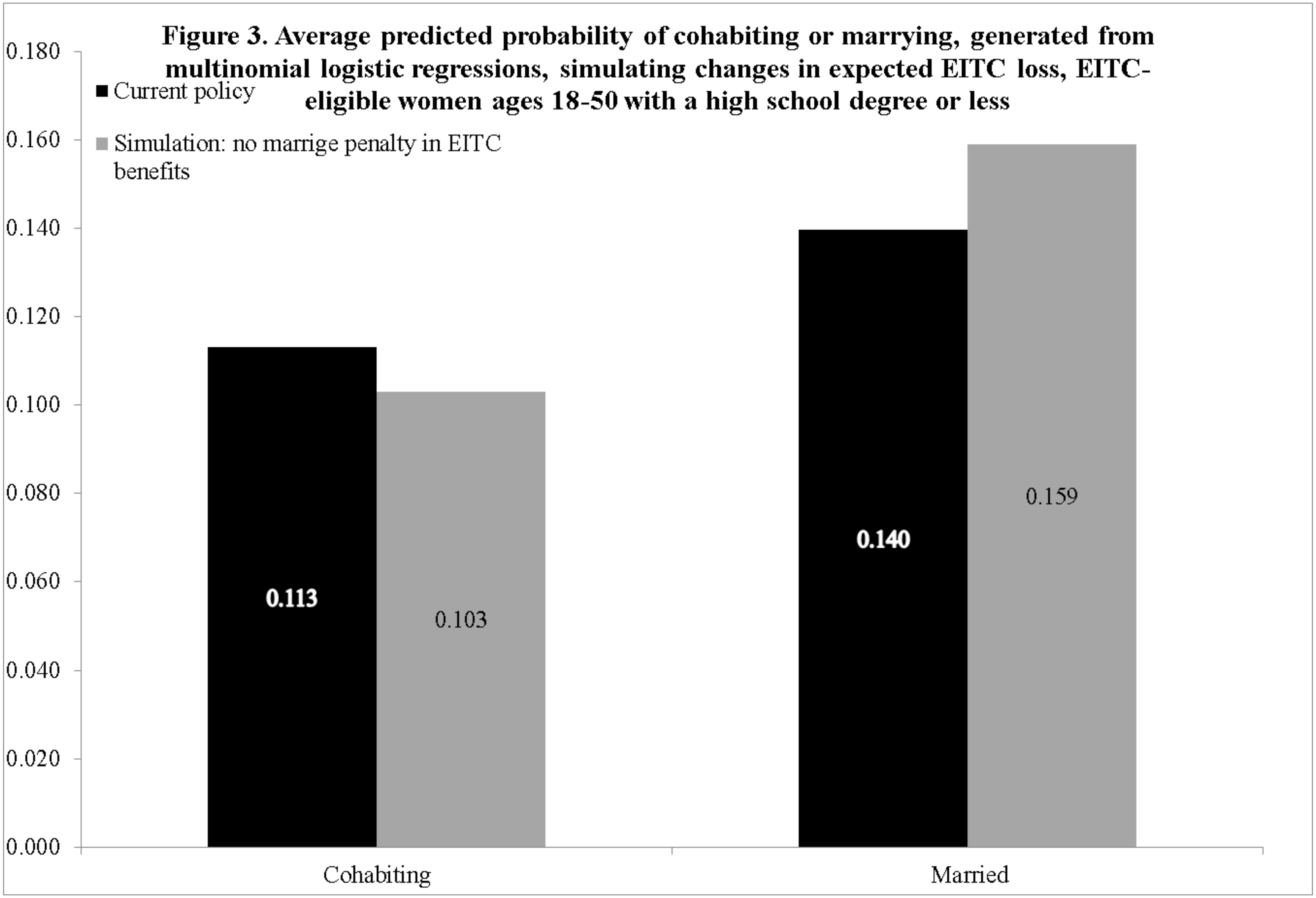


Figure 3. Average predicted probability of cohabiting or marrying, generated from multinomial logistic regressions, simulating changes in expected EITC loss, EITC-eligible women ages 18-50 with a high school degree or less

Table A1. Characteristics of Spouses of Married Women in 2001, 2004 and 2008 SIPP, by Race and Education

	White, Non-Hispanic											
	<HS						HS					
	19-24	25-29	30-34	35-39	40-49	50+	19-24	25-29	30-34	35-39	40-49	50+
Observations	448	707	750	861	2,103	2,449	1,819	3,597	4,776	6,541	16,243	16,192
Spouse Characteristics												
Spouse's Age												
19-24	41.3%	5.1%	1.1%	0.6%	0.0%	0.0%	43.7%	4.2%	0.4%	0.2%	0.1%	0.0%
25-29	39.5%	41.0%	8.4%	2.0%	0.3%	0.3%	39.7%	43.9%	7.2%	1.0%	0.2%	0.1%
30-34	14.1%	33.1%	39.3%	5.0%	1.1%	0.4%	10.2%	36.3%	41.1%	7.9%	1.1%	0.2%
35-39	3.6%	11.2%	32.8%	37.9%	7.6%	0.7%	2.9%	10.1%	35.7%	41.7%	5.7%	0.5%
40-49	1.6%	7.4%	15.3%	46.8%	60.4%	8.4%	1.9%	4.7%	14.0%	44.6%	65.0%	8.0%
50+	0.0%	2.3%	3.1%	7.8%	30.5%	90.2%	1.5%	0.8%	1.7%	4.5%	27.9%	91.1%
Spouse's Race												
White	87.3%	88.3%	90.5%	93.4%	92.2%	95.0%	88.9%	90.7%	93.2%	93.6%	95.0%	95.6%
Black	1.8%	1.6%	1.5%	0.9%	0.9%	0.9%	1.6%	1.8%	1.1%	1.2%	0.9%	0.6%
Hispanic	8.5%	7.9%	6.1%	3.6%	4.4%	2.4%	7.2%	5.4%	3.8%	3.4%	2.6%	1.9%
Other	2.5%	2.3%	1.9%	2.1%	2.6%	1.6%	2.3%	2.1%	1.9%	1.7%	1.6%	1.9%
Spouse's Education												
Less than HS	43.8%	44.0%	47.2%	41.2%	41.2%	41.5%	10.4%	9.0%	8.9%	9.0%	8.0%	9.2%
High School Deg	44.2%	36.8%	37.5%	40.0%	40.0%	39.5%	62.7%	55.0%	58.2%	56.2%	55.3%	50.7%
Some College	9.6%	15.8%	12.5%	14.1%	13.4%	14.3%	21.5%	27.2%	23.3%	22.8%	23.4%	24.9%
College	2.5%	3.4%	2.8%	4.8%	5.5%	4.7%	5.3%	8.7%	9.6%	11.9%	13.3%	15.2%

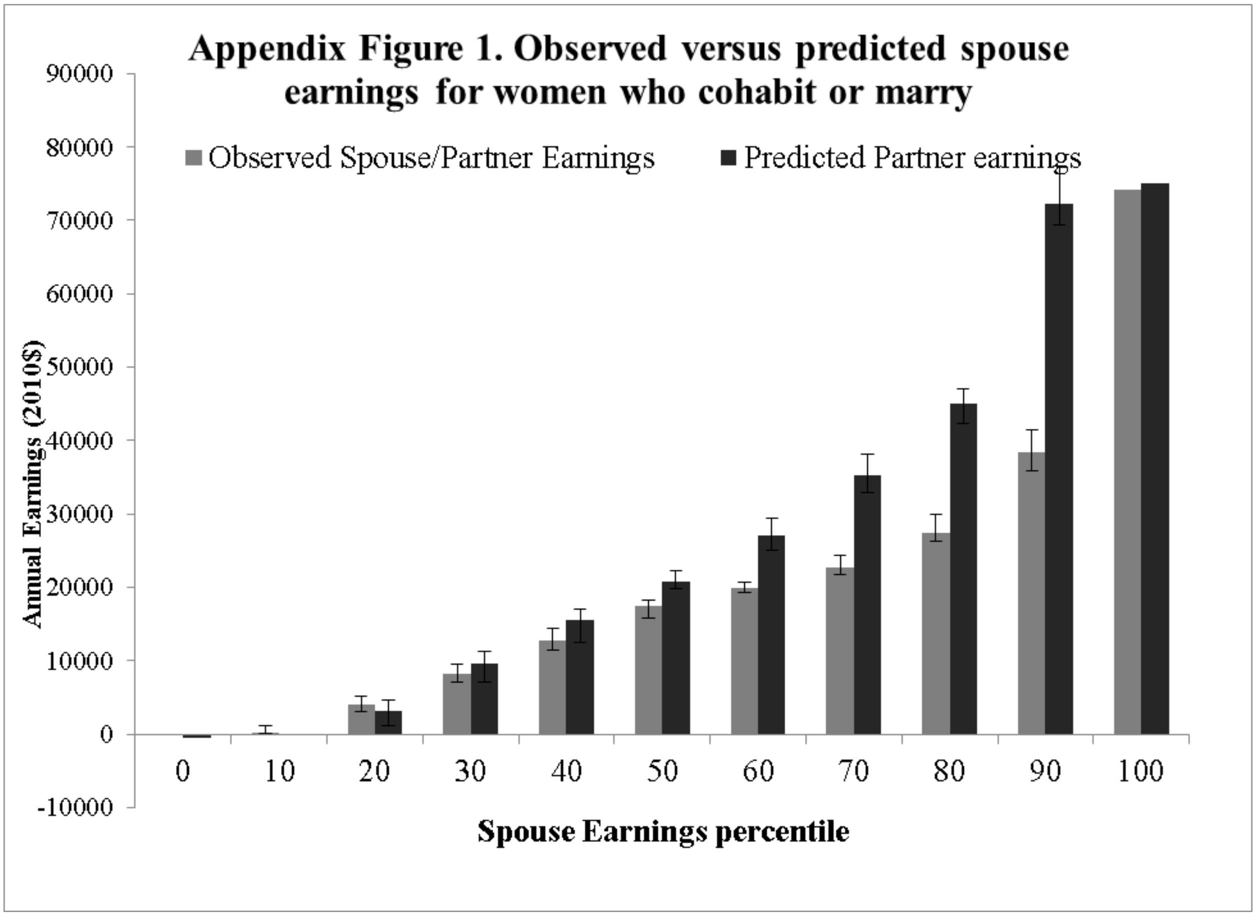
Source: 2002-2011 CPS population of married women and their spouses, aged 18-65

	Black Non-Hispanic											
	<HS						HS					
	19-24	25-29	30-34	35-39	40-49	50+	19-24	25-29	30-34	35-39	40-49	50+
Observations	59	72	104	141	366	589	147	356	489	642	1,653	1,680
Spouse Characteristics												
Spouse's Age												
19-24	39.0%	5.6%	2.9%	0.7%	0.0%	0.0%	42.2%	5.1%	0.8%	0.5%	0.0%	0.1%
25-29	28.8%	26.4%	5.8%	1.4%	0.5%	0.0%	33.3%	39.0%	10.8%	1.9%	0.5%	0.2%
30-34	11.9%	40.3%	29.8%	5.7%	2.5%	0.3%	13.6%	27.2%	36.6%	12.0%	1.8%	0.5%
35-39	10.2%	9.7%	26.0%	27.7%	9.8%	1.0%	3.4%	17.1%	28.4%	37.9%	7.4%	1.0%
40-49	8.5%	13.9%	27.9%	55.3%	48.1%	9.0%	4.8%	8.1%	18.6%	40.2%	57.8%	10.1%
50+	1.7%	4.2%	7.7%	9.2%	39.1%	89.6%	2.7%	3.4%	4.7%	7.6%	32.6%	88.1%
Spouse's Race												
White	6.8%	5.6%	3.8%	5.7%	2.7%	1.7%	6.1%	5.9%	3.48%	4.05%	2.72%	2.26%
Black	91.5%	90.3%	92.3%	92.2%	95.1%	96.6%	88.4%	92.4%	92.84%	92.99%	95.40%	96.49%
Hispanic	1.7%	4.2%	2.9%	0.7%	1.4%	0.8%	2.0%	1.4%	2.66%	1.56%	1.33%	0.83%
Other	0.0%	0.0%	1.0%	1.4%	0.8%	0.8%	3.4%	0.3%	1.02%	1.40%	0.54%	0.42%
Spouse's Education												
Less than HS	44.1%	37.5%	39.4%	35.5%	43.7%	55.2%	8.8%	7.9%	7.98%	11.68%	10.28%	16.90%
High School Deg	44.1%	44.4%	47.1%	48.9%	34.7%	34.0%	65.3%	58.4%	65.24%	60.59%	59.04%	55.24%
Some College	6.8%	15.3%	11.5%	12.1%	14.8%	9.0%	20.4%	23.0%	16.97%	19.16%	22.69%	19.76%
College	5.1%	2.8%	1.9%	3.5%	6.8%	1.9%	5.4%	10.7%	9.82%	8.57%	7.99%	8.10%

Source: 2002-2011 CPS population of married women and their spouses, aged 18-65

	Hispanic											
	<HS						HS					
	19-24	25-29	30-34	35-39	40-49	50+	19-24	25-29	30-34	35-39	40-49	50+
Observations	834	1,560	2,160	2,100	3,140	2,253	793	1,403	1,669	1,627	2,551	1,878
Spouse Characteristics												
Spouse's Age												
19-24	30.2%	6.5%	1.2%	0.9%	0.3%	0.2%	39.8%	5.0%	1.1%	0.4%	0.1%	0.2%
25-29	44.8%	35.5%	9.3%	2.4%	0.6%	0.1%	40.2%	41.9%	9.3%	1.5%	0.4%	0.3%
30-34	15.7%	37.2%	37.8%	9.6%	3.3%	0.9%	14.2%	36.3%	42.8%	12.4%	2.8%	0.5%
35-39	4.9%	14.1%	34.3%	40.3%	7.6%	1.2%	3.3%	12.1%	30.9%	43.2%	8.5%	1.0%
40-49	2.9%	6.0%	15.5%	42.0%	59.5%	11.5%	1.6%	3.6%	14.4%	37.6%	64.1%	11.7%
50+	1.4%	0.7%	1.9%	4.9%	28.7%	86.0%	0.8%	1.1%	1.5%	5.0%	24.1%	86.4%
Spouse's Race												
White	3.36%	2.37%	1.99%	2.76%	3.22%	3.99%	10.47%	11.69%	11.74%	14.87%	20.27%	21.88%
Black	0.84%	0.38%	0.19%	0.19%	0.25%	0.18%	0.76%	0.93%	1.86%	1.48%	1.45%	1.22%
Hispanic	95.44%	96.79%	97.55%	96.71%	96.21%	95.47%	87.52%	86.10%	85.20%	82.48%	77.34%	74.60%
Other	0.36%	0.45%	0.28%	0.33%	0.32%	0.36%	1.26%	1.28%	1.20%	1.17%	0.94%	2.29%
Spouse's Education												
Less than HS	71.82%	74.42%	75.28%	77.05%	75.10%	74.43%	29.26%	26.51%	23.07%	21.63%	20.34%	20.29%
High School Deg	20.26%	18.65%	17.69%	15.29%	15.64%	15.31%	52.21%	53.60%	53.86%	54.33%	50.06%	47.02%
Some College	5.64%	5.58%	5.19%	5.24%	6.43%	7.63%	13.87%	14.40%	16.18%	17.09%	19.33%	20.29%
College	2.28%	1.35%	1.85%	2.43%	2.83%	2.62%	4.67%	5.49%	6.89%	6.95%	10.27%	12.41%

Source: 2002-2011 CPS population of married women and their spouses, aged 18-65


Appendix Figure 1. Observed versus predicted spouse earnings for women who cohabit or marry
Observed Spouse/Partner Earnings
Predicted Partner earnings
Annual Earnings (2010$)
90000
80000
70000
60000
50000
40000
30000
20000
10000
0
-10000
0
10
20
30
40
50
60
70
80
90
100
Spouse Earnings percentile

CHAPTER 3

FERTILITY PATTERNS OF COLLEGE GRADUATES BY FIELD OF STUDY, U.S. WOMEN BORN 1960—79 (WITH KELLY MUSICK)

The fertility patterns of U.S. college graduates are increasingly distinct from those of women with lower levels of education. Whereas women of all education levels have postponed marriage, only college-educated women have delayed childbirth to the same extent as marriage (Ellwood and Jencks 2004; McLanahan 2004). The gap in age at first birth has grown, and by the end of their reproductive years, college-educated women are more likely to be childless and have fewer children overall (Rindfuss et al. 1996; Martin 2004; Musick et al. 2009). Investigators of family formation processes in the United States have tended to focus on the early childbearing of women with the lowest levels of education (e.g., Ribar 1999; Furstenberg 2003; Carlson et al. 2004; Edin and Kefalas 2005), and there has been relatively little research on the pattern of later and lower fertility that is characteristic of U.S. college graduates. The continuing increase in college enrolments among women in the U.S. and other advanced industrialized countries (Buchmann and DiPrete 2006; Bhrolchain and Beaujouan 2012) makes it important to obtain a better understanding of variation in the effects of college study on family life.

Different fields of undergraduate study lead to career trajectories that differ in their economic rewards, demands, and the relative importance of employment and family. For this reason, field of study might be expected to be an important factor in explaining fertility variation among college graduates, and research in Europe has already begun to explore this association (Lappegård 2002; Lappegård and Rønsen 2005; Hoem et al. 2006a, 2006b; Martín-García and Baizán 2006; Neyer and Hoem 2008; Van Bavel 2010). Studies there find that fertility is indeed highly stratified by field of study. Subsequent childbearing is at least as closely associated with field of study as level of education in Norway (Lappegård 2002; Lappegård and Rønsen 2005), Spain (Martín-García and Baizán 2006), and Sweden (Hoem et al. 2006a, 2006b). To our knowledge, no systematic investigation of links between field of study and fertility has been undertaken in the United States.

In the study reported in this paper, we built on recent European research with the aim of obtaining a better understanding of variation in the fertility patterns of U.S. college-educated women. The expansion of women's educational attainment and employment in the United States and Europe has unfolded in different labor markets and has been subject to different social policies. European welfare regimes are relatively generous in their support of women's labor force participation, with such provisions as paid family leave, subsidized child care, and part-time work (Gornick et al. 1997; Waldfogel 2001; Gornick and Meyers 2003). The United States ranks low in work-family support policies, but there may be trade-offs in greater flexibility in labor markets, gender equality in access to jobs and pay, and cheaper private-sector child care (Morgan 2005; Mandel and Semyonov 2006; Pettit and Hook 2009; Mandel and Shalev 2009a, 2009b). These trade-offs may favor college graduates (Mandel and Shalev 2009a, 2009b; Mandel 2010). Despite weak support policies, labor force attachment and fertility rates in the U.S. remain high relative to those in Europe (Brewster and Rindfuss 2000; Morgan 2003; Morgan 2005; Misra et al. 2011).

We drew on large, nationally representative samples from the 2001, 2004 and 2008 Survey of Income and Program Participation (SIPP) to provide the first analysis of fertility differences between groups of U.S. college graduates by their undergraduate field of study. We used multilevel event-history models to investigate potential mechanisms linking field of study to delayed fertility and childlessness. To explore institutional and selection processes, models included the following indicators: motherhood employment penalties; percentage of men in the field; early marriage (as measured by the SIPP); and early attitudes about family roles (as measured by the National Longitudinal Survey of Youth 1979, (NLSY79)).

II. Background

II.A How might field of study matter?

A growing line of research focuses on the potential importance of institutional accommodations for easing the competing demands of work and family on women's time (Bianchi 2000; Joshi 2002; DiPrete et al. 2003; Morgan 2003; Rindfuss et al. 2003; Morgan and Taylor 2006). The idea is that the easier it is for women to combine motherhood and employment—rather than having to choose between them—the weaker the constraints on childbearing. Indeed, at the aggregate level, the long-held negative relationship between women's labor force participation and completed fertility has reversed in developed countries: high rates of women's participation in the labour force are now associated with high fertility (Brewster and Rindfuss 2000; Billari and Kohler 2004).

Conditions that reduce work-family conflict include greater flexibility and smaller penalties for time spent outside the labor force (England 1992; Glass and Camarigg 1992; Goldin and Katz 2008b). We assessed workplace accommodations by measuring the differences in labor force participation between mothers with children aged under five, and all other women, by their field of study. We postulated that fields of study leading to jobs with smaller motherhood employment penalties would impose fewer constraints on childbearing and result in earlier and higher overall fertility.

Institutional perspectives suggest causal mechanisms linking field of study and family formation patterns, but there are undoubtedly also selection processes at work. Hakim (2000) has emphasized the importance of heterogeneity in women's lifestyle preferences—particularly the degree to which women are home or work-centered—for understanding women's fertility decisions. She makes the following proposals: 'home-centered' women obtain education as a form of social capital; ‘work-centered’ women invest heavily in training geared specifically to careers; and a middle group of ‘adaptive’ women obtain education with a view to working, although investing less than the ‘work-centered’. The ‘adaptive’ group might particularly be expected to select fields of study based on their perception of the ease of balancing work and family obligations in the jobs characteristic of those fields. The extent to which women are family-centered may also be associated with individual characteristics. For example, nurturance and a preference for working with people might select women into such caring or helping professions as teaching, health, and social work (Fortin 2008; Folbre 2010). These selection processes may be reflected in the gender composition of fields and attitudes about family roles typical of fields.

Any effects of field of study on fertility may be indirect, working through variation in age at marriage. Fertility remains closely associated with marriage among U.S. college graduates, with only 7 per cent of births in this group occurring outside marriage (Kennedy and Bumpass 2011). Consequently, differences in marriage rates across fields of study may be a key factor in shaping variation in childlessness. Fields of study associated with less stable career trajectories or lengthier training periods may delay marriage (Oppenheimer et al. 1997), as might fields with weaker ties to particular jobs (Hoem et al. 2006b). Further, fields of study may provide different opportunities for marriage, either promoting or inhibiting it. For example, fields in which a high proportion of students are men may shape marriage prospects via the availability of prospective partners, with more prospective partners leading to earlier marriage and, in turn, childbearing. This suggests that the percentage of men in a field may reflect more than selection into fields; it also points to potential nonlinearities in the relationship between percentage of men and childbearing.

II.B Previous research

A handful of studies in Europe have explored the association between field of study and fertility. Hoem et al. (2006a, 2006b) found that Swedish women who studied health and teaching had lower rates of childlessness and higher overall fertility than women who studied in other fields. Similarly, studies in Austria (Neyer and Hoem 2008), Norway (Lappegård 2002; Lappegård and Rønsen 2005), and Spain (Martín-García and Baizán 2006) all found earlier and higher overall fertility among women in fields of study related to caring or helping professions. As a factor in differentiating fertility, field of study generally seems to be equally or more important than educational level. For example, among Swedish women with the equivalent of a college degree, 10 per cent of those who had graduated in health and teaching were childless, whereas the largest proportion childless amongst all other fields of study was 30 per cent, a difference of 20 percentage points. The maximum difference in childlessness across educational levels was only 5 percentage points (13 per cent among those with less than a high school education and 18 per cent among those with high tertiary education, or college degrees). In contrast, among U.S. women aged 40–44 in 2008, the difference in childlessness across educational levels was substantially higher at 9 percentage points: 15 per cent of those without a high school degree were childless, compared with 24 per cent of those with a college degree (U.S. Census Bureau 2010).

European data show that childlessness is highest among women who studied the arts and humanities, followed by those who studied the social sciences, and mid-range for women in science and technology (Lappegård 2002; Hoem et al. 2006b; Neyer and Hoem 2008). There is much concern in the U.S. over the small proportion of women in science and technology, and considerable debate over how family roles and preferences influence behavior (Ceci and Williams 2007; Ceci et al. 2009; Sassler et al. 2011; Morgan et al. 2013). Women graduating in disciplines with large concentrations of men are generally more likely to remain childless and tend to have fewer births on average, although there are exceptions. Hoem et al. (2006b) has suggested that the relatively weak ties to future occupations in the arts and humanities (which typically offer no special job training or teaching qualifications) might explain the high rates of childlessness in these fields, despite their relatively high levels of female representation. How the fertility experiences of U.S. women in science and technology fields compare with those of European women is unknown.

Van Bavel (2010) further explored how field of study relates to fertility postponement across 21 European countries, focusing on percentage of men, family attitudes, and earnings potential as mechanisms linking field of study to fertility. Using multilevel conditional probability models with women cross-classified by country and field of study, his analysis found childbearing at earlier ages among those who had graduated in fields of study characterized by a higher representation of women and more traditional family attitudes. It also found later childbearing among women in fields of study with higher earnings potential (as indicated by the expected starting wage and the steepness of the rise in earnings with age), a finding consistent with the hypothesis that higher wages mean higher income forgone in the event of childbirth, and thus higher opportunity costs of having children. While a significant factor in Van Bavel's study of women across educational levels and countries, we expected wages to play a weaker role in differentiating fertility by field of study among our sample of U.S. college graduates, for whom wages were relatively homogeneous (this was borne out by sensitivity tests that included information on wages by field of study).

In sum, evidence from Europe suggests that fertility differences by field of study are driven by a mixture of both causal and selection effects. That is, field of study appears to have a causal effect on attitudes and career prospects (and, in turn, fertility behavior), and women appear to be selected into fields of study by their attitudes about family life. Women's attitudes evolve with age and experience (Thornton et al. 1983; Fan and Marini 2000), and Van Bavel's measure of family attitudes was assessed while men and women were in college, when their attitudes were possibly already shaped by the different social environments of fields of study.

We know of no general investigation of the relationship between undergraduate field of study and fertility in the U.S. The closest is work by Goldin and colleagues, which documented fertility differences among elite college graduates by advanced degree and career trajectory. Comparing three cohorts of women who had graduated from Harvard with advanced degrees, they found that family size was generally smallest for those with a doctorate, and largest for physicians (Goldin and Katz 2008b). Physicians took the shortest time off after having a child and experienced the smallest loss of earnings for time off. Goldin reported similar results from the College and Beyond data, which included graduates who entered a broader set of selective colleges in 1976 (Goldin 2006). She and Katz concluded (2008b, p.7) that 'women in careers with the greatest predictability and the smallest financial penalty for time out have the most children'. Characteristics of careers are more directly related to the factors that could ease or exacerbate work-family conflict than fields of study. At the same time, careers are established much further into the life course, and decisions about what job to take or even whether to work may be affected by family formation. Our focus on field of study allows us to examine part of the career process that takes place well before the birth of the first child for the vast majority of college graduates.

Given the lack of research on the relationship between field of study and fertility in the U.S., how might one expect that relationship to compare with the findings for Europe? As noted above, the United States ranks low relative to Europe in work-family support policies, such as paid leave and subsidized child care, but high in labor market flexibility (e.g., Gornick and Meyers 2003; Mandel and Semyonov 2006). An unintended consequence of paid-leave policies appears to be lower wages and greater occupational segregation by gender, which disproportionately affect highly skilled women (Mandel 2010; Misra et al. 2011). The prospect of motherhood may thus be less constraining to U.S. college graduates than to their counterparts in the more developed welfare states of Western Europe. The U.S. educational system is also less rigid, lacking the strong vocational and apprenticeship programs of many of the European systems (Goldin and Katz 2008a; Mandel and Shalev 2009). The more general approach to education and to the greater overall flexibility in job opportunities in the United States suggest that fields of study there might be less tied to specific occupational characteristics, potentially resulting in smaller differences in fertility patterns among undergraduate fields of study.

II.C Our approach

We explored how field of study relates to fertility delay and childlessness among U.S. college-educated women, relying primarily on data from the SIPP. Our investigation of potential mechanisms linking field of study to fertility included measures of selection processes discussed in past European work, as well as indicators of institutional accommodations not examined elsewhere. Our analyses focused on college graduates because there is little educational specialization before college enrolment in the U.S. (the European studies mentioned above included women of different education levels). Data from the SIPP were available on the timing of first births to women only, precluding analysis of men's fertility. The SIPP was well suited to our study, with samples large enough to investigate variation by field of study and also to provide detailed information on fertility, education, earnings, employment, and marriage.

We start with a descriptive background and then explore the potential factors linking field of study with family formation. We estimated discrete-time multilevel event-history models of first birth among women aged 20–48 as a function of individual-level socio-demographic variables and field of study-level characteristics. Most of the latter were generated from data on women aged 21–55 years in the SIPP, including motherhood employment penalties, percentage of men, and early marriage. Family role attitudes were assessed using data from the NLSY79, a panel survey of 14–21-year-olds that began in 1979. Attitudes were observed at the first wave of data collection, before college enrolment for most, and thus largely before respondents were influenced by their experiences in particular fields. Thus we had a relatively good measure of attitudes as a selection factor into fields of study. We further explored the role of differences across fields in marriage timing (per cent married by age 26) in explaining differences in the timing of first births. Finally, we used our model results to predict how the proportion childless would change when varying field-of-study-level characteristics.

Our data and modeling approach allowed us to examine the institutional, selection, and intervening variables perspectives outlined earlier. These were our starting hypotheses: (1) Institutional accommodations that reduce the cost of combining motherhood with employment may weaken constraints on childbearing, implying that smaller motherhood employment penalties are positively associated with the transition to a first birth. (2) Women may select into fields of study on the basis of characteristics that also predict earlier transitions to motherhood; for example, traditional family values may lead to earlier first births and draw women to the (female-dominated) caring and helping fields. The percentage of men in a field may further reflect partner availability, potentially resulting in nonlinearities in the relationship between field of study and fertility. (3) Marriage timing may play a mediating role in the link between field of study and fertility; in particular, the proportion of women married by age 26 is likely to be associated with earlier births. These processes need not be competing; each may be occurring simultaneously, some potentially offsetting, and others reinforcing. Of course, our measures are only rough proxies; we discuss their limitations in greater detail in the discussion section.

III. Data and method

III.A Survey of Income and Program Participation

The SIPP is a multi-part survey conducted in the U.S. every four months through in-person interviews with all individuals aged over 15 in the household (U.S. Census Bureau 2009). For the rounds of the survey we used, the number of households and the periods over which they were followed were as follows: 36,700 over 36 months in 2001; 46,500 over 48 months in 2004; 52,000 households over 40 months in 2008. The primary purpose of the SIPP is to gather information about sources of household income, but information on specific topics is also collected in separate modules. We relied on the second module for retrospective fertility and marital histories. Also included in the module were details of schooling, including degrees attained, timing of degree completion, and characteristics of educational programs. SIPP person-weights account for differential non-response and panel attrition and were applied in all descriptive analyses.

We restricted our analysis to women respondents born 1960–79 (aged 20–48 at interview) who had completed a four-year college degree by age 25 and were childless at the completion of their degree. (Characteristics of the field of study were generated from more general samples, described in greater detail below). Excluding women who did not finish their degree by age 25 resulted in a loss of about 15 per cent of the sample of women with college degrees, and excluding those who had their first child before completing their degree resulted in an additional loss of 5 per cent. These exclusions resulted in a sample of women with college degrees who were slightly more educationally advantaged than the overall population of women with college degrees, which somewhat limited the representativeness of our results, but ensured that the field of study was completed before the first birth. Pooling sample numbers from the 2001, 2004 and 2008 SIPPs yielded a total sample of 8,895 women. We transformed the dataset into a file of person-years at risk for our event-history analysis, with one record for every year of age from degree to first birth, giving a total of 79,664 person-years.

The 1960–79 birth cohort provided a large sample of women with college degrees and allowed us to follow some to the end of their fertile years. At the same time, this 20-year span represents a fairly recent snapshot of college graduates and their fertility. The oldest of them were coming of age in the late 1970s, when increases in college enrolment, labor force participation, fertility postponement, and childlessness were well under way (Bianchi 2000; Martin 2004; Goldin et al. 2006). We did not consider earlier cohorts whose experiences in school, work, and family were quite different (Goldin 2004).

III.B National Longitudinal Survey of Youth (NLSY79)

The NLSY79 is a nationally representative panel survey that has followed nearly 13,000 men and women aged 14–21 in 1979, representing the 1958–65 birth cohort (U.S. Bureau of Labor Statistics 2008). The NLSY79 includes attitudinal questions that are not in the SIPP, but the sample sizes are considerably smaller (with just 1,258 women college graduates), making it difficult to examine fertility patterns even for broad fields of study. Supplementing our main data file with data on attitudes by field of study from the NLSY79 allowed us to combine its detail with the large samples of the SIPP. We used a sample of 3,572 women who reported a college field of study by the 2008 wave. We aggregated the more detailed fields of study reported in the NLSY79 to match SIPP definitions.

III.C Measures

Fertility. Women aged over 15 were asked the number of children ever born and the year of their first and most recent births. Lacking information on the timing of men's fatherhood or women's intermediate births (between the first and the most recent), our analysis was restricted to women's first births.

Field of study. Individuals who had completed a bachelor's degree indicated which of 18 major fields of study they had undertaken in school. We eliminated individuals in the pre-professional fields of study, whose numbers were too small to analyze separately. We included data on the remaining 17 specific fields of study in our final models to maximize variation but aggregated them into 7 broader categories to present descriptive patterns: arts and humanities; education; general studies; health sciences; private and public administration; science and technology; and social sciences (see detailed description of fields in the Appendix). These broad categories are consistent with definitions from past work (Hoem et al. 2006a, 2006b; Neyer and Hoem 2008; Van Bavel 2010). One limitation with this categorization, also pointed out in European studies, is that we were able to identify women in teaching only if they had graduated in education. It is possible that women in other fields, such as history or English, subsequently attained teaching qualifications.

Characteristics of field of study. We aggregated the data on working-age college graduates (aged 21–55) from the 2001, 2004, and 2008 SIPPs to generate all the field-of study-level variables except family role attitudes (which we derived from the NSLY79). The 'motherhood employment penalty' was measured as the difference between the percentage of labor force participation of mothers with children aged under five and that of women with no children under five. Percentage of men was measured simply as the percentage of men having graduated in a given field of study, and we included the squared percentage of men in the field as well to allow for a curvilinear association between percentage of men in the field and women's fertility. Finally, the timing of early marriage was assessed by the percentage of women married by age 26.

We derived a measure of family role attitudes specific to field of study from NLSY79 women reporting a field of study. Relevant questions were asked in 1979 and 1982; we relied on the answers to the 1979 questions but substituted 1982 responses if missing. Respondents were asked to indicate their agreement with the following, ranging from 1 = strongly disagree, to 4 = strongly agree: a woman's place is in the home, not the office or shop; a wife with a family has no time for outside employment; employment of wives leads to more juvenile delinquency; it is much better if the man is the achiever outside the home and the woman takes care of the home and family; men should share the work around the house with women; and women are much happier if they stay home and take care of children. We reverse-coded the item referring to men sharing work around the house, omitted individuals who failed to answer 3 or more of the 6 items, and averaged over given answers to produce an index of traditional family role attitudes (estimated reliability of $\alpha = 0.91$), with higher values corresponding to more traditional family role attitudes. We used the standardized version of this index in the multivariate models to facilitate interpretation, so that change was measured in standard-deviation units. Because this measure came from a different survey with smaller sample sizes than the main survey, it is likely measured with more error, which may attenuate estimated effects. Women in the NLSY79 sample were also slightly older than the women in our main sample. As attitudes were becoming more progressive over this period (Thornton et al. 1983), we may have captured somewhat more traditional family role attitudes in data from the NLSY79. Nonetheless, barring rapid *differential* change in attitudes across fields, our measure should capture relevant differences between fields of study. Finally, we used a larger sample of college-goers (3,572), rather than of college graduates (1,258), to estimate family role attitudes for each field of study, so providing more stable estimates; the results were not sensitive to this slight change in definition.

Individual-level demographic controls. We generated time-invariant indicators of the respondent's race and ethnicity: non-Hispanic White; non-Hispanic Black; and Hispanic. We also constructed a time-invariant quantitative variable for the year in which the respondent obtained her bachelor's degree to account for cohort differences in fertility patterns.

III.D Multilevel event-history model

We examined the timing of first birth with a discrete-time multilevel event-history model that nests individual person-years within field of study. Our baseline duration was a function of age, specified as categories to allow for flexibility in fertility patterns by age: 20–24; 25–29; 30–34; 35–39; and 40 and over. All models included time-invariant individual-level controls (race/ethnicity and year of degree) and field-of-study-level motherhood employment penalties, percentage of men,and family role attitudes. Some models included field-of-study-level indicators of marriage timing. The model used can be written as a basic logistic function, in terms of the log odds of a first birth:

$$\ln [(p_{ijt} / (1 - p_{ijt})] = \gamma_{00} + \gamma_1 \mathbf{A}_t + \gamma_2 \mathbf{X}_{ij} + \gamma_3 \mathbf{Z}_j + \upsilon_{0j} \quad (1)$$

where p_{ijt} is the probability that individual i, in field of study j, has a first birth in person-year t; $\mathbf{A}_t$ is a vector representing the five age categories specified above; $\mathbf{X}_{ij}$ is a vector representing our time-invariant individual-level controls; and $\mathbf{Z}_j$ is a vector of field-level characteristics. These covariates are represented by fixed slopes γ_1, γ_2, and γ_3, respectively. The element γ_{00} is an overall intercept term, and υ_{0j} is a field-specific random error term with variance σ_μ^2, intended to capture heterogeneity across fields of study unexplained by our covariates (Teachman 2011).

We generated model-based predicted probabilities to illustrate results in a more intuitive way than is possible with logits or odds ratios. Applying the estimated coefficients and sample means to a transformation of equation (1), we calculated age-specific predicted probabilities of first birth. We then multiplied these conditional probabilities together to yield the predicted probability of ever having a first birth—the complement being the probability of being childless. In this way, we can show differences in both the timing and incidence of childbearing across fields of study. Using results from full models pooled over fields of study, we altered the values of the field-of-study-level covariates to simulate variation in the proportions permanently childless. These simulations add further detail to the substantive key findings.

IV. Results

IV. A Descriptive results

First birth timing and incidence. We ran field-of-study-specific discrete-time event-history models of first birth to explore differences in the timing of fertility among women using our main sample (timely college graduates born 1960–79, aged 20–48 at interview, and childless on graduating). We regressed the logit of first birth on a cubic term for age to impose some structure on our descriptive results (weighting models and including no other controls). We used model estimates as described above to generate predicted age-specific probabilities of first birth. Figure 1 shows predicted probabilities of childlessness at age 44 by field of study. The smallest predicted probabilities of childlessness occur for women in education and health, at 16.4 and 18.5 per cent, respectively. The largest probabilities occur for women in the arts and humanities (25.2) and general studies (26.4 per cent). Childlessness among women in administration (22 per cent), science and technology (22.1 per cent), and social sciences (23.1 per cent) take intermediate values. Differences are generally statistically significant across these three groupings, and overall patterns are similar to those found in Europe. The overall predicted rate of childlessness is 22.4 per cent. This estimate is reasonably close to the value of 24 per cent for the proportion childless among U.S. college graduates, as estimated from the CPS (U.S. Census Bureau 2010). The difference between the two estimates reflects differences in measurement; in our study, the probability is a period-based estimate derived by cumulating age-specific rates of first birth over the childbearing years, whilst the estimate derived from the CPS is a cohort one, based on completed fertility of women aged 40-44 in 2008. (The estimate derived in the present study therefore reflects the fertility of slightly earlier cohorts).

Figure 2 gives more detail on how age-specific rates of first birth cumulate to estimates of the eventual proportions with children, and those without them, at age 44. Women in education and health make the earliest transitions to first birth, with higher birth probabilities than other college graduates until their early 30s, when probabilities fall and attain a rate of increase similar to that of other fields of study. Still, because these women start childbearing earlier, they remain more likely than others to have had a child at each age and less likely to be childless at age 44. Women who studied science and technology and public administration appear to have low probabilities of first births early on but relatively high probabilities in their mid to late 30s, resulting in moderate levels of childlessness. Women who completed degrees in arts and humanities also have fewer births in the early years, but without any obvious 'catch up' later. From regression models (not reported here), we found that women in science and administration have a significantly smaller chance of having a first birth than women in education, but only for ages under 30. In contrast, women in all other fields of study have significantly smaller chances at all ages of having a first birth than women in education.

Field characteristics. Table 1 summarizes the key characteristics of the different fields of study that might account for the association between field of study and fertility: motherhood employment penalties; percentage of men; and traditional family attitudes.

If those fields of study associated with higher fertility (i.e., education and health) are more accommodating of a work-family balance, we would expect to see smaller employment penalties for having young children, i.e., a smaller gap in employment between mothers with young children and other women. The data partly confirm this expectation: the penalty is smallest for health (9.2 percentage points), followed by education (12.3 percentage points). The education and health fields of study are heavily dominated by women (sixth row, Table 1), with men accounting for only 22.9 and 20.7 per cent of degrees, respectively. In contrast, the science and technology field is heavily dominated by men, who earn 70.6 per cent of such degrees. Except for the difference between education and health, all differences in percentage of men among broad fields are statistically significant. We found that the percentage of men in a given field of study correlated positively with traditional family role attitudes characteristic of fields, with education and health (the most women-dominated fields) also scoring highest on traditional attitudes (1.98). Women in general studies, private and public administration, science and technology, and social sciences are all significantly less traditional than women on average in their family attitudes. Finally, women in education and health were more likely to be married by age 26: 54.4 and 46.4 per cent, respectively. These figures are significantly higher than the 37 to 39 per cent of women married by age 26 in all other fields, consistent with the earlier first births and lower levels of childlessness in education and health.

IV.B Multivariate results

Table 2 gives the odds ratios from our discrete-time multilevel event -history models. Odds ratios indicate how changes in a given covariate are associated with changes in the odds of having a first birth, with values above 1 indicating a positive association and those below 1 a negative association. The top panel shows covariates at the individual level, including age, degree year, and race and ethnicity. The bottom panel shows field of study-level characteristics for 17 specific fields (rather than the 7 broad fields used in our descriptive summary, above). Table 2 presents the results of three models: Model 1 includes only demographic information at the individual level, Model 2 includes our key field-of-study-level variables, and Model 3 adds indicators of early marriage. All models include a random effect at the field-of-study level to allow for unobserved correlations within fields of study, and the associated standard deviation (shown at the bottom of Table 2) represents heterogeneity across fields unexplained by controls.

Socio-demographic variables in Model 1 operate as expected (and change little across models): the odds of a first birth are largest for women aged 30 to 34, and Hispanic graduates are 1.3 times more likely to have a first birth at any given age than non-Hispanic Whites (with the odds for Blacks being statistically insignificantly different from those for Whites). Adding characteristics of the fields of study in Model 2, we find that motherhood employment penalties are not significantly associated with the odds of having a first birth: that is, our measure of work-family inflexibility does not, contrary to our expectation, appear to constrain family formation. Model 2 shows a statistically significant association between the percentage of men in a given field of study and the odds of a first birth. The association, however, is not linear: the results for the percentage of men suggest that the concentration of men in a field of study is negatively associated with the probability of having a first birth, but that the strength of the association declines at higher concentrations. As expected, we also find a positive association between traditional family attitudes and first-birth probabilities. A one-standard-deviation increase in traditional attitudes is associated with a 6 per cent increase in the odds of a first birth.

Finally, Model 3 adds the indicator for marriage timing for each field of study, to examine the extent to which field characteristics are associated with fertility through the intervening life event of marriage. Not surprisingly, the proportion married by age 26 is positively associated with the odds of having a first birth. After accounting for the timing of marriage, the coefficient on the percentage of men in the field of study becomes statistically insignificant, and the coefficient for traditional family attitudes becomes smaller (although remaining statistically significant). These findings suggest that these mechanisms may indeed operate at least partly through differences in marriage timing; in particular, it appears that marriage is important in accounting for the association between the percentage of men in a field and fertility.

As noted, all models include a random effect at the field-of-study level to allow for unobserved heterogeneity within fields of study, and standard deviations of this term are presented at the bottom of Table 2. With no field-of-study-level controls, the standard deviation of the random intercept at the field level is 0.14. Once we add our key field-level controls in Model 2, the standard deviation falls to 0.078, indicating that field-level characteristics account for about one half of the variation across fields of study. Finally, once we include the proportion of women married by age 26, the standard deviation falls to 0.023, indicating that our field-level controls account for the vast majority of the variation in the timing of first births across fields of study. By comparison, in a model controlling for age, education, and a country-level intercept, but no field characteristics, Van Bavel (2010) reported a field-level standard deviation of 0.22, which changed little with additional field-level controls. The contrast provides some evidence of stronger field-level differences in Europe than in the U.S., but the difference may be explained by Van Bavel's broader sample of women across education levels and countries.

IV.C Simulations

To illustrate our findings in more intuitive terms, we ran simulations of childlessness based on Model 2 (Table 2) results, before marriage was included as an intervening variable. For each of our significant field-level variables, we predicted levels of childlessness at age 44, varying field-level characteristics at their minimum, median, and maximum values, and holding all other covariates at their mean values. Figure 3 illustrates the results of this exercise. Modeling the percentage of men as a quadratic captures the non-linear relationship between the percentage of men in a field and the timing of the first birth. There is a larger proportion of childless women in fields with the median percentage of men than in fields of study with either the minimum (health: 21 per cent men) or the maximum (engineering: 86 per cent men). The difference in childlessness between fields at the minimum and maximum values of percentage men is only 2 percentage points, while the difference between fields at the minimum and the median values is nearly 9 percentage points. There is a larger difference in proportions childless by family role attitudes: a difference of 13 percentage points between those fields of study with the least and those with the most traditional attitudes about family roles.

V. Discussion

We set out to examine variation in the timing and occurrence of first births among U.S. college graduates by undergraduate field of study and, further, to explore mechanisms by which field of study might influence fertility. To our knowledge, this was the first study to provide an analysis of childbearing patterns by field of study in the U.S.

In initial analyses, we found significant differences in the timing and occurrence of first births across fields of study. Women who studied education and health were the earliest to have a first birth, while women in science and technology appeared to follow a pattern of delay and catch-up. The smallest proportion childless at age 44 were women in education and health, for whom the proportion was 10 percentage points smaller than for graduates in arts and humanities and general studies. The proportions childless for women in science and technology, social studies, and administration took intermediate values. These patterns are consistent with those reported in Europe (Lappegård 2002; Hoem et al. 2006b; Neyer and Hoem 2008), though we were surprised by the relatively large probabilities of first birth for women in science and technology, a field of study dominated by men, where the sex imbalance receives considerable attention and sparks much debate in the United States (e.g. Ceci and Williams 2007).

We estimated multilevel event-history models to assess the importance of field-of-study characteristics in accounting for individual-level variation in becoming a mother. We postulated that fields of study leading to jobs with smaller motherhood penalties—measured by differences in employment between women with young children and all other women—should impose fewer constraints on childbearing. These smaller penalties may reflect workplace accommodations that make it easier to combine work and family, and overall, penalties may signal to childless women information about the career costs of childbearing. We found little support for these possibilities, at least as modeled, with no significant association between motherhood employment penalties and first-birth timing.

We used the percentage of men within fields study and traditional family role attitudes typical of fields of study as proxies for individual characteristics like nurturance, preferences for working with people, and a pro-family orientation that potentially select women both into the 'caring' fields and earlier motherhood. Our measure of family role attitudes (derived from the NLSY79) was assessed early in the life course (at ages 14-21), largely before any potential influence of experiences in fields of study on attitudes, thus providing a reasonably straightforward selection criterion. Because this measure was generated from a different data source than our main sample, it was probably measured with more error, which would have tended to attenuate estimated effects. We nevertheless found that traditional family role attitudes were strongly associated with earlier age of first birth, with simulations showing a 13 percentage point difference in the proportions childless between the least traditional and the most traditional fields of study, ceteris paribus. Differences remained statistically significant after including controls for marriage patterns by field of study.

The percentage of men within a field of study was also significantly associated with first-birth timing, although both the pattern and interpretation of the association were somewhat more complicated. We found a curvilinear relationship: childlessness was greater in fields with intermediate percentages of men than in fields with either a small or a large percentage. The curvilinear pattern is consistent with larger probabilities of first birth among women in science and technology, a field heavily dominated by men (over 70 per cent), than among women in such fields as arts and social sciences in which the percentages of men are intermediate (about 40 per cent). Van Bavel (2010) reported a negative relationship between proportion of men and age at first birth across Europe, but no account was taken of potential nonlinearities. Exceptions to the general rule of higher fertility in more women-dominated fields have been discussed elsewhere (e.g., Hoem 2006b). We noted earlier the possibility that a large proportion of men may reflect selection processes, but may also mean greater partner availability and a marriage market that clears more quickly for women. For example, the presence of a large number of men with similar interests may result in women in science and technology marrying earlier than women in fields with smaller numbers of male graduates. Approximately 40 per cent of women in science and technology were married by age 26—larger than the proportions in arts and humanities, general studies, or social sciences. The proportion of men in a field of study could also reflect differences, not taken into account by other controls, in the association between field and subsequent career. For example, a positive association between field of study and predictable, stable career paths might explain the relatively small proportion childless among women in science and technology (despite its small proportion of women) (Hoem 2006b; Goldin and Katz 2008b).

Our results suggest that marriage timing plays an important role in mediating the link between field of study and fertility, with early marriage associated with earlier first births. Early marriage appears to account fully for the relationship between the percentage of men in a field of study and fertility: controlling for the proportion married by age 26 in Model 3 made the coefficient on the percentage of men statistically insignificant. The coefficient on family-role attitudes for fields of study fell in magnitude, although it remained statistically significant. Of course, given the very low rate of non-marital fertility among U.S. college graduates, it is not surprising that field-of-study characteristics operate substantially through marriage timing.

The United States ranks low relative to Europe in work-family support policies, such as paid leave and subsidized child care, but high in terms of flexibility of educational systems and labor markets (e.g., Gornick and Meyers 2003; Mandel and Semyonov 2006, 2009; Goldin and Katz 2008a). But despite these differences, our findings are largely in line with European studies (e.g. Hoem et al. 2006a, 2006b, Neyer and Hoem 2008, Van Bavel 2010). That is, the pattern of childlessness across fields of study is similar, although the differences between the fields of study appear to be smaller in the U.S. For example, in the U.S., the smallest estimated proportions childless were among women in education and health (16.4 and 18.5 per cent, respectively) and the largest were for women in the arts (25.2 per cent) and general studies (26.4 per cent), resulting in a difference across fields of study of 10 percentage points. By contrast, the analogous comparison in Sweden is approximately 10 per cent childless in education and health and 30 per cent in the arts and humanities, giving a difference of 20 percentage points (Hoem et al 2008b).

We have relied on informative data with relatively large samples to provide the best evidence to date on U.S. fertility differences across fields of study and on plausible explanatory mechanisms. Nevertheless, there are limitations in the data and the approach. We found some support for institutional and selection mechanisms, but causal pathways were difficult to identify. The SIPP contains limited individual-level variables relevant to fertility decisions, and unobserved factors may have confounded our estimated associations. The field-of-study measures we derived were merely proxies for factors that might explain how field of study influences fertility. For example, we used motherhood employment penalties to assess institutional features of the occupations characteristic of each particular field of study, but a more detailed examination would need to include more direct indicators, such as parental leave policy, job flexibility, and other aspects of working conditions (not measured by the SIPP). Further, field of study characteristics are themselves largely proxies for future career paths. It is likely that fields of study differ in the heterogeneity of occupations chosen after graduation.

Relatively little research has been devoted to the increasingly distinctive fertility of U.S. college graduates. Our findings lend support to the notion that women's choice of field of study is based on factors that simultaneously predict earlier motherhood, namely, traditional family values. Our findings also highlight the importance of marriage timing in accounting for differences in fertility across fields of study. This analysis serves as a starting point for a better understanding of the relationship between the institutional factors that potentially constrain or facilitate family formation and the selection factors that shape women's outlooks on work and family.

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Table 1. Characteristics of fields of study of U.S. women graduates (from SIPP) and fields of study of women completing some U.S. college (from NLSY79), women born 1960–79

	Arts and humanities	Education	General studies	Health sciences	Private and public administration	Science and technology	Social sciences	Overall
2001, 2004, 2008 Survey of Income and Program Participation								
Motherhood employment penalty	15.70	12.30	13.90	9.20	15.80	15.20	16.30	14.00
Per cent men	42.20	22.90	50.20	20.70	55.80	70.60	36.70	50.2
Per cent of women married by 26	37.00	54.40	37.20	46.40	39.00	39.80	37.20	41.4
1979 National Longitudinal Survey of Youth								
Traditional family attitudes	1.92	1.98	1.88	1.98	1.92	1.86	1.85	1.92
Number of Observations (NLSY)	294	406	194	590	1200	534	354.0	3572
Number of Observations (SIPP)	2,586	3,100	3,166	1,527	3,555	2,183	2,103	18,220

Note: Motherhood employment penalty was calculated as the difference in labor force participation rates between women with children under the age of five and all other women in each field. Per cent men and motherhood employment penalty were calculated using 2001, 2004 and 2008 SIPP samples of college graduates aged 21–55. Attitudes from the NLSY79were measured in 1979, when individuals were 14–21 years old. 1982 attitudes were used when 1979 values were missing.

Source: 2001, 2004, and 2008 Survey of Income and Program Participation (SIPP) and the 1979 National Longitudinal Survey of Youth (NLSY79). SIPP women aged 21–55 who had obtained at least a bachelor’s degree by SIPP interview. NLSY79 women aged 14–21 in 1979 who had completed at least some college course by the 2008 interview

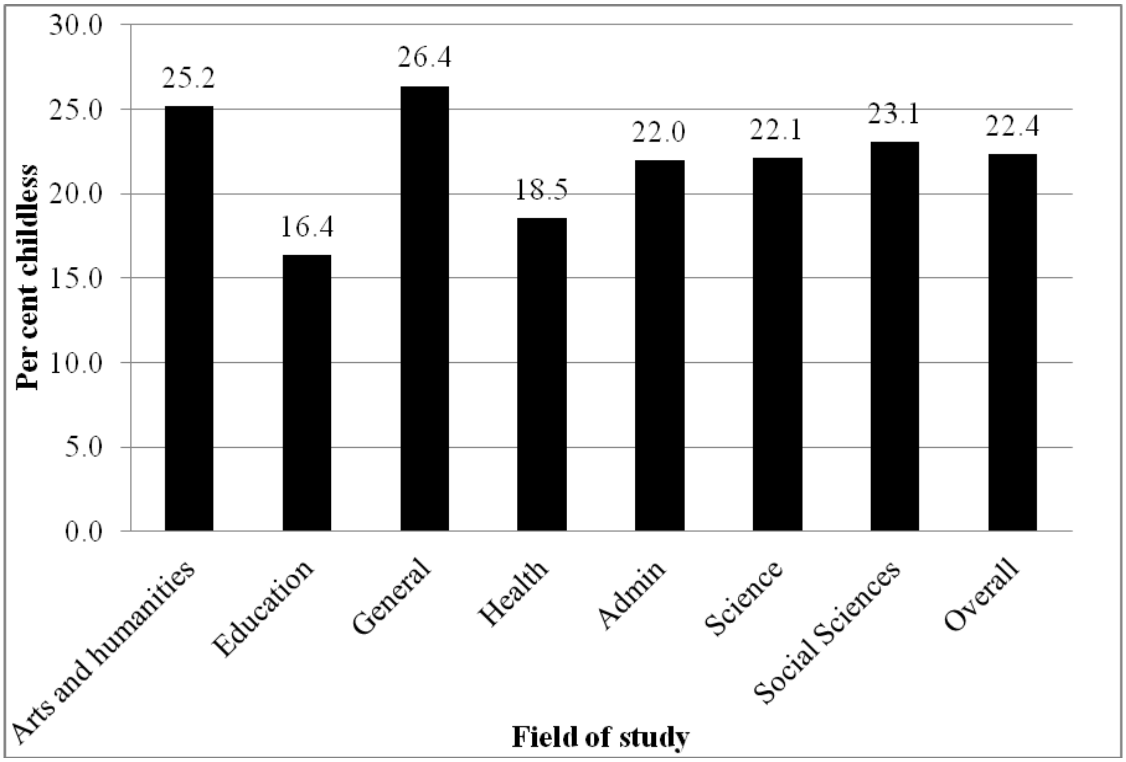
Table 2. Odds ratios of individual and field-of-study characteristics associated with having a first birth, from discrete-time event-history models, U.S. graduate women born 1960–79, aged 21–48

	M1: Demographics		M2: Field characteristics		M3: Full model	
Individual characteristics						
Degree year	0.998		0.998		0.998	
	(0.003)		(0.003)		(0.003)	
Race (White non-Hispanic reference group)						
Black, non-Hispanic	0.957		0.960		0.963	
	(0.058)		(0.058)		(0.058)	
Hispanic	1.289	***	1.290	***	1.290	***
	(0.105)		(0.105)		(0.105)	
Age 20-24	0.207	***	0.207	***	0.207	***
	(0.010)		(0.010)		(0.010)	
Age 25-29	0.652	***	0.652	***	0.652	***
	(0.022)		(0.022)		(0.022)	
Age 30-34 (reference)						
Age 35-39	0.648	***	0.647	***	0.647	***
	(0.037)		(0.037)		(0.037)	
Age 40 and over	0.184	***	0.184	***	0.184	***
	(0.026)		(0.026)		(0.026)	
Field of study characteristics						
Motherhood employment penalty			1.000		1.000	
			(0.008)		(0.006)	
Per cent men			0.974	***	0.998	
			(0.007)		(0.007)	
Per cent men squared			1.000	***	1.000	
			(0.000)		(0.000)	
Per cent women married by age 26					1.020	***
					(0.004)	
Traditional family attitudes (from NLSY79)			1.058	**	1.042	**
			(0.025)		(0.019)	
Random effects intercepts						
Field of study (standard deviation)	(0.143)		(0.078)		(0.023)	
Wald Chi squared (df)	1123.9		1142.75		1239.91	
Observations	79,664		79,664		79,664	

Note: Family attitudes are based on 6 questions in the 1979 National Longitudinal Survey of Youth about attitudes towards family roles. See text for description of questions. Scale runs from 1–4 with higher values indicating more traditional family role attitudes. Measure was then standardized for ease of interpretation. A one-unit increase in the traditional family role attitudes represents a one standard deviation increase in traditonal family role attitudes.
Source: 2001, 2004 and 2008 Survey of Income and Program Participation, sample of women graduates with no children at timely degree completion, born 1960–79.

*p < 0.10 **p <. 0.05 ***p < 0.01

Figure 1. Predicted probabilities of childlessness by field of study, derived from field-specific discrete time event-history models of first birth, U.S. graduate women born 1960–79



Note: Separate, weighted models run for each field of study. Logit of first birth regressed on cubic function of age.

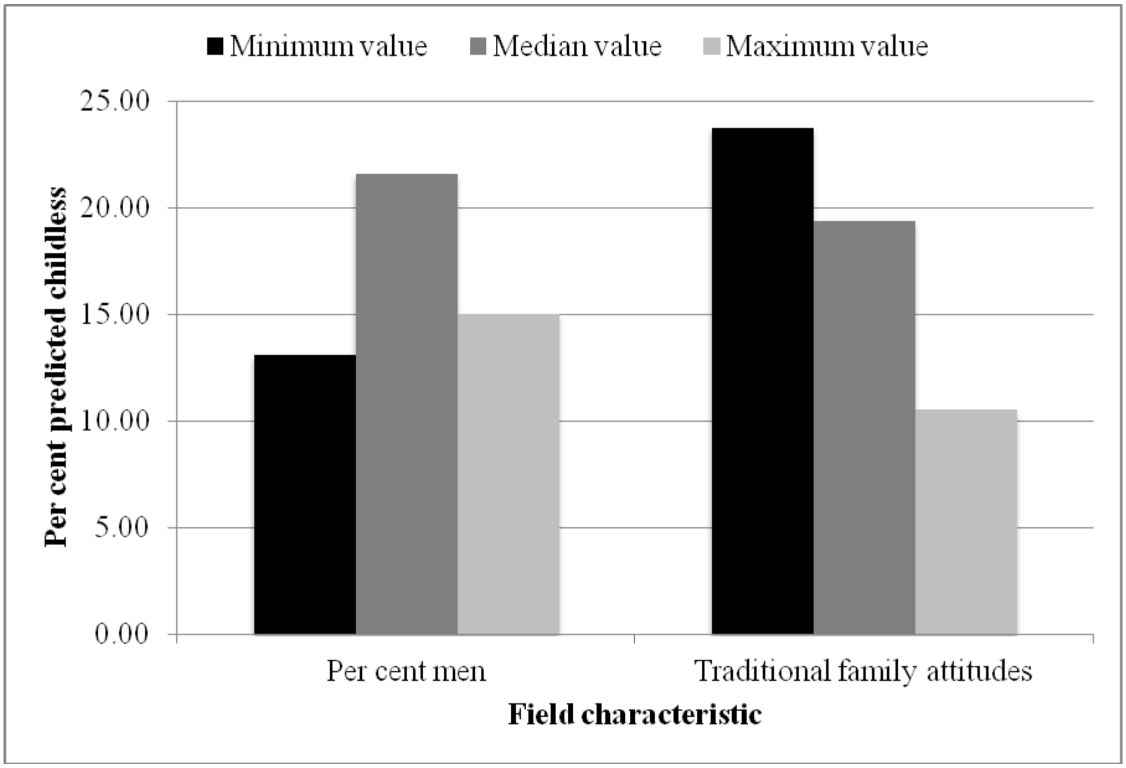
Sources: 2001, 2004, and 2008 Survey of Income and Program Participation, sample of women graduates with no children at timely degree completion.

Figure 2. Cumulative predicted probabilities of first birth for U.S. graduate women by age and field of study, derived from field-specific discrete time event-history models of first birth, U.S. graduate women born 1960–79

Note: Separate, weighted models run for each field of study. Logit of first birth regressed on cubic function of age.

Sources: 2001, 2004, and 2008 Survey of Income and Program Participation, sample of women graduates with no children at timely degree completion.

Figure 3. Predicted probability of childlessness at age 44 for women, derived from Model 2, varying key field-level characteristics, U.S. graduate women born 1960–79



Note: Estimates generated from Model 2 in Table 2. Field-level characteristics in turn set to their minimum, median, and maximum values while all other covariates held at their mean values. The minimum value for the traditional family attitudes variable reflects women with non-traditional attitudes towards family roles, so they exhibit higher levels of childlessness. This corresponds with our findings from Model 2 that suggest that higher values of the traditional family attitudes variables correspond to lower rates of childlessness.

Source: 2001, 2004, and 2008 Survey of Income and Program Participation, sample of women graduates with no children at timely degree completion.